

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

EDITED BY
 PROF. GILBERT RYLE

WITH THE CO-OPERATION OF PROF. SIR F. C. BARTLETT AND PROF. C. D. BROAD

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I.—G. E. MOORE

ON October 24th there died at Cambridge George Edward Moore, O.M., Litt.D., Hon.Ll.D., F.B.A. Born in 1873, he began his teaching career in Cambridge in 1911, becoming Professor of Philosophy in 1925. He retired from his Chair in 1939. He took over the editorship of MIND from G. F. Stout in 1921, and continued to edit this Journal until 1947. He was nearly 85 years old when he died.

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II.—SENSATIONS AS GUIDES TO PERCEIVING

BY ROY WOOD SELLARS

SOME forty years ago there was a movement in American philosophy called *critical realism*, which reached co-operative expression in a book, *Essays in Critical Realism*. It had two varieties, one of which stressed essences while the other thought in terms of concepts whose application in perceiving was guided by sensations. Both forms were *referential* rather than *inferential* in their outlook. That is, they thought of perceiving as involving a directed reference to physical things in an objective way, tied in with behaviour or action, rather than as being an affair of inference to them from sense-data. The aim was the establishment of a direct realism as against the Lockian tradition of an indirect, or representative, realism.

Not unnaturally, the essence-variety got the most attention—especially abroad—because of the prestige of Santayana and C. A. Strong. I belonged to the other group. Since I have had a long time to reflect on the problems involved and have, to my own satisfaction at least, clarified the issues, I thought it would be well to present my version of critical realism in its relation to modern biology and psychology and bring it to bear upon current controversies. It seems to me, for instance, to give a *framework* for the recent, linguistic approach without involving extreme behaviourism, as understood in England.¹

American new realism was a form of presentational realism which, by a *tour-de-force*, more or less suggested by William James, gave objectivity to sensory presentations and rejected the category of the subjective. It was often called pan-objectivism and was associated with the kind of behaviourism which rejected any private stream of consciousness. Montague, who was one of the moving spirits, regarded it as a transformation of Humianism by giving an objective status to his sense-impressions.

R. B. Perry was, in many ways, the best expositor of this outlook. His form of philosophical behaviourism cut beneath

¹ It may be of historical interest—to set the record right—to call attention to the fact that I gave the name to the movement, having started to call myself a critical realist in 1908 in a series of articles in the *Journal of Philosophy* and having published a book with that title in 1916. Durant Drake, the editor of the *Essays* carried through the adoption. Dawes Hicks used the term later and quite independently.

Cartesian dualism by rejecting a subjective "consciousness". In this fashion, American new realism went further than English neo-realism with its transparent acts. Russell was intrigued by its rejection of "consciousness".

All this represented a gambit of a daring sort. I do not, myself, think it reflected as careful an analysis of the perceptual experience as the critical realists were making. The line we took was that perceiving was a referential activity in which objects were characterized. Thus, I point denotatively to *this* chair before me and conceptually characterize it as brown, having four legs, etc. Knowing is, here, not a searchlight terminating on the presentational given but more of a propositional claim guided by sensory factors. It is this functional relation of sensations and cognitive claims which makes it *sense-perception*. But more about this later.

In this period of American philosophy, Cartesian dualism was being pretty generally attacked. I had rejected it in an article in the *Psychological Review* in 1907. This was in a controversial paper directed against Mark Baldwin under whom I had studied. Dewey swung from idealism—never favourable to Cartesian dualism—to a kind of biological experimentalism. I do not think he was ever strong in epistemology which he thought was connected with Cartesian dualism. The psychologists were moving toward a sort of methodological behaviourism with reservations as to the standing of introspection. At Michigan, where we had a flourishing school of experimental, comparative psychologists, *Gestalt* ideas came increasingly into favour. I would speak of these psychologists as *Gestalt* behaviourists. That is, they took the organism as the unit and studied it in any available way. So far as rats were concerned, they could only observe experimentally controlled behaviour but they were ready to use *analogies* from the human level and even spoke of rats as *reasoning* when they met new situations and solved problems. Later, I came into contact with C. Judson Herrick, the distinguished comparative neural anatomist, who calls himself a behaviourist but thinks of behaviourism as including mentation of different levels.

Now all this just leads up to the point that philosophers were apt to be more puristic in their use of terms than biologists and psychologists are. This is, perhaps, because they have their own problems in mind. Professor Ryle shifted to a linguistic development of philosophical behaviourism with his rejection of the "ghost in the machine". He was, obviously, not fond of epistemology and was vague about the status of sensations and images. It was an interesting gambit which he could later

qualify, as he seems to be doing at present. I shall say more about this later.

My referential view of sense-perception¹ allowed me to develop what I called a double-knowledge theory of the brain-mind. We can know it externally as the neurologist does and as its functioning is schematized by the student of behaviour who regards it as, *par excellence*, the organ of behaviour. And each one can regard his personal experiencing as intrinsic to this functioning. This is the position taken by a neuro-surgeon, like Penfield, who tries to correlate his subject's descriptive comments with his surgical information. I was early led to speak of personal consciousness as a "natural isolate which was experienced personally and could be correlated with the functioning of the brain as known *about* referentially."² This is my double-knowledge.

Now this signifies that epistemology can help to throw light upon the mind-body problem. After all, the philosopher should make use of his tools. But he should keep in touch with scientific developments.

Any knower has to start from his perceptual experience and its referential claims. There is *no* knower in general.

Thus we must come back to a study of the nature and conditions of perceptual experience, as our starting-point. The new realist simplified the issue by his presentationalism and his rejection of private consciousness (the subjective). The English neo-realist had his *act* of awareness left over but, so far as I could make out, did not pay very much attention to the mind-body problem. But the critical realist had before him the possibility that perceiving was a complex operation guided by sensations (personal) and terminating in behavioural and linguistic activities of a referential sort which could be objectively observed and communicated. Guided by my visual field, I point to *this* chair, walk towards it, make statements about it. What we call concepts have a close connection with both language and behaviour. But I take it to be fairly evident that, in sense-perception, this terminal complex is guided by sensations. It is *not* the sensations which are the objects of perception but the material things we are concerned with in behaviour and language, yet they function in the background. G. E. Moore was always bothered, I think, by this functional duplicity and worried over

¹ As far as I know, Miss Stebbing came nearest to this position in England. I could never see where C. D. Broad got his substance and causality to add to sense-data. He still seemed to begin with the data.

² Those who may be interested will find an account of this view in the *Aristotelian Society Proceedings* (1923).

the question whether sense-data are parts of the surface of objects perceived. Now, as we shall see, the critical realist did not start with sense-data but with the objective reference and claims of perception; and he was open-minded as to the "how" of the operation. Perceiving seemed to him a referential operation tied in with behaviour and language. Thus to be knowledge *about*; and not a presentational *givenness* of the object. In short, a confirmable claim in terms of evidence, prediction, and successful fitting into conduct. The *complexity* of the perceptual operation is recognized, at the level of commonsense, by such terms as 'appears', 'looks'. Psychology is apt to shift to visual fields as against the physical fields. But, in so doing, it tends to neglect the cognitive claim. It is the job of the philosopher to try to clarify the situation. But he needs an adequate epistemology as a point of departure.

But before I go further and examine perceiving as an achievement and its "how" or mechanism, I must say a few words about the climate of philosophy and its zigzag in the intervening years. There were slogans, proposals, logical accomplishments, a sort of ferment; but, so far as I could see, neglect of epistemology as a little *sinuos* or the pronouncement that idealism and realism had reached a stalemate which should be politely recognized. Needless to say, I did not like it at all. But I was ready to welcome any technical advances. On the whole, I regard the linguistic development as an advance which fitted into my notion of objective reference. I describe linguistically what I am pointing at. Language is not tied up to *acquaintance* and special "proper names", as it tended to be with Russell. Gradually, the natural *context* and *use* of language was recognized. But, in my opinion, a corresponding epistemology had not been worked out. I shall try to show that critical realism offers the framework.

And so I say good-bye to logical positivism, phenomenalism, constructionism, the pathetic belief that a "powerful logic" would somehow act as an Archimedean lever. On the whole, I think the English fell less for the magic of technical manipulation than did the Americans. And so I am going to connect up my story with the articles by Ryle, Ayer, Barnes and Hampshire in the volume entitled *Contemporary British Philosophy* which was sent to me for a brief review. Since I was a contributor to the American series of the thirties, my analysis may have historical interest. At least, it may call attention to a neglected possibility. Since phenomenalism seems to have shot its bolt, realism should come in for exploration again. Wilfrid Sellars used to tell me that the positivists were becoming realists but did

not *like* the word. But now my friend, Feigl, calls himself a critical realist and declares that Schlick was much of that outlook before he came under the hypnotic influence of the Wittgenstein of the *Tractatus*.

I shall not give pagination references to the articles mentioned but concern myself with their purport. Ryle seems to be worried by the status and role of sensations; Ayer is defending direct realism against scepticism; Barnes stresses the factor of personal experience in perceiving—I think rightly; and Hampshire is concerned with the status of a bright spot on the horizon *which* we judge to be a ship, which, as I see it, involves the role of sensory factors as guides in perceiving. What can the critical realist say about these points? In a way, I have already given my answer but I want to illuminate it by a detour. This will take up the so-called causal theory of perception which, historically, was the generator of the whole notion that we start, in perceiving, with sense-impressions, or sense-data, as isolated atoms and have the job of inferring external things from them, something which both Berkeley and Hume saw could not be done. Unfortunately, instead of challenging the schematism (which, I shall try to show, had to wait for developments in modern biology and psychology) they resorted to subjective idealism, phenomenalism-*cum*-scepticism (however you wish to interpret Hume), etc. Mill never got beyond the traditional framework.

Now the causal theory of perception was a theory of the “how” of perceiving which led philosophers down the garden path or, to use the everyday expression of the rat-psychologists, into a blind alley. It made our actual, perceptual beliefs untenable.

What was wrong? The model was *not* that of biology or modern psychology in which the *unit* is a *pattern* of stimulus *and* response integrated together or a sensori-motor circuit, as it is also called. Now the sensori-motor circuit is, in man and the higher animals, an open, or growing, one into the making of which *learning* enters. The correct formula is S-C-R. The stimulus gets its functional significance from its integration with R through C. The point is that there are levels of learning and integration. Centrally aroused processes, C, mediate between stimulus and response so that the stimulus becomes a *cue* to adjusted responses. It is important to stress such growth in the adequacy of responses to objects and situations, an affair of exploratory learning, as well as the fact that the stimulus is used according to this enlarged setting into which it plays. There has been a work of discrimination and interpretation going on. This is what I meant by saying that the whole pattern is the functional unit.

Now I do not think that I need to go into the technicalities of the present neuro-psychological view. Let it suffice to say that, as we pass up from the level of fairly fixed, sensori-motor patterns to those which permit modifications, we find this role played by cortical processes in building up responses under guidance, now very complex factors of guidance, which act as cues, or indications. What dominates the operation is the need, or motivation, of the percipient; and this is directed not so much at the stimulus, as at the object to which one is being alerted. The percipient is looking at something, listening to, exploring, feeling around. The stimulus is the *cue* to a directed and developed response, not, as a rule, something of moment for its own sake. But attention can be shifted to the sensations, as in delayed responses; and this is very characteristic of man.

What I want to do now is to show the bearing of all this upon the causal theory of perceiving, that is, on the "how" of perceiving. I think it can be shown that this outlook does not, as the traditional one did, result in a model which frustrates objective perceiving as an achievement and shuts one into an awareness of sense-data as atomic facts from which one can escape only by a postulated, inferential act which seems to have no empirical base. Instead, the *how* of perceiving now supports, and throws light upon, the directed claims of perception, as an *achievement*. You cannot have an achievement without a mode in which it is accomplished. Neither science nor philosophy can be satisfied with the one without the other. Fortunately, I think, the time is ripe for their linkage.

But, before I re-examine the modern, realistic movement with its strong shift to the acceptance of *direct realism* as a starting-point, I want to bring out the point that the traditional model of the causal theory of perception was tied in with both an emphasis upon sense-data, as the primary objects, and with Cartesian dualism. I need not go into this in historical detail for it is too well known. The *scheme* was an impact upon the brain through the sense-organs and a consequential rise in the soul-mind of sensations which the soul-mind had the capacity to become aware of. Hume, of course, allowed this scheme to drop out of sight but he never quite accounted for awareness. But the point I want to make is that sensations were never linked up with response in a patterned way. The outlook was not biological but introspective and artificial. It was as though responses were so delayed, or ignored, that attention had shifted to sensations for their own sake. Their normal role as *cues* to learned patterns of response was disregarded. That was one of the great

faults of traditional, introspective psychology which led to the revolt—carried to an extreme, at first—of behaviourism. The development of a solution of the mind-body problem which would free it from Cartesian dualism, emphasize patterns of the S-C-R type, and yet find a place for personal consciousness would take time. Personal consciousness would, of course, have to be localized in connection with the *alertedness* involved in the development and running off of patterns which are not automatic. Now an adequate epistemology would make for clarification here. But, unfortunately, philosophers were, on the whole, either still dominated by the schematism of the traditional model in some of its repercussions or inclined to adopt "behaviourism" as a way of escape from Cartesian dualism. The ingenuity exhibited was terrific. But it would seem that the period of second-thought is, at last, upon us.

I am now ready to draw my conclusions and to show that critical realism had, in the main, the correct approach. The test will be epistemological, primarily, though I shall try to show that perception, as an achievement, fits in with the "how" of perceiving, as studied by neurologists and psychologists, and with what I have long called the emergence, double-knowledge outlook on the mind-body problem. This latter is anti-Cartesian, behaviouristic, in a broad sense, but not reductive or blind to the facts of personal experience. It fits more into what I have called *Gestalt* behaviourism. What I have always been opposed to are Procrustean methods. I wanted to keep in mind the whole picture. I have often thought that philosophers missed the suggestions offered by developments in science. Analysis, alone, tends to be a case of trying to lift oneself by one's bootstraps. To solve problems one must make hypotheses and use insights, just as science does. Waismann is quite right here; and I was glad to note Ryle's cartographic analogy in the Cambridge book.¹ As my study of perceiving in its "how" and its achievement has, I believe, shown, philosophy has more than linguistic confusions to clear up. It must help to solve problems in co-operation with the sciences. And that is not speculation in the old sense.

I merely mention the fact here that Russell, in his latest phase, has resorted to a causal approach founded on physics. Ayer is, I think, quite right in his comment on it. My objection to it is that it did not probe the mechanism of perceiving itself. My friends told me that Russell had become a critical realist but that

¹ *British Philosophy in the Mid-Century*, ed. Mace, The MacMillan Co.

was only partially so. He had not pressed through an attack upon the epistemological problem, itself, and fell back on postulates.

Let me turn now to some comments on the English writers I have mentioned as probing perceiving. I shall be brief.

Ayer seems now to accept direct realism. For him, as for me, sensations function as guides, or cues, and, alternately, as evidence for the characterization of material things, a characterization which can be read off linguistically and be the basis of prediction and adjustment. His objection to naïve realism is that it accepts results without asking their nature or how they are arrived at. Hence, it is apt to fall back on the word *intuition* without having any clear idea as to what it involves.

That has always been my objection to intuitionism, in ethical theory as well. I early followed up my analysis of cognition with one of valuation in which I stressed appraisal as a guided ascription of value in terms of the role the object plays in personal and social life, value not being a *property*, natural or non-natural, but a justifiable appraisal. Criteria are used in the ascription. This seems to be a growing outlook. But here, also, I would stress the distinction between the "how" and the achievement. These things hang together in their variety. But merely to cognize is not to value, though appraisal makes use of cognition. And there is more than "emotive meaning" here. Concepts which go with attitudes operate. But I must stop at this point.

My query with respect to Ayer is this, Has he come to realize the point that perceiving terminates in a directed knowledge-claim which can be made explicit in demonstrative and descriptive words but has the *setting of looking at, listening to, feeling, handling*, etc.? This does not involve the presentation-presence of the thing which has become a referent. This is the import of *aboutness*. While the percipient *has* sensations and *uses* concepts the object perceived is regarded as co-ordinate with the percipient and something to adjust to. But, of course, the percipient can perceive himself but he is not then *merely* having sensations and using concepts. He is perceiving his body.

Ayer is so clear and careful an expositor that I hope much from this turn his thinking has taken. I remember that, as a young man, he lectured at the University of Michigan. Needless to say, he had not even heard of the critical realism movement. Despite the best efforts, philosophy does not move with an even front.

Probably as a protest against the extreme position of linguistic behaviourism Ryle had taken in his revolt against Cartesian dualism, Barnes is defending the concomitance of personal

experience with the objective reference and characterization involved in perceptual statements. I have argued that personal seeings and hearings function, in a guiding way, as parts of the larger pattern which acts as a *directed unit* and terminates in attitudes, tendencies to behaviour, pointing, with words like *this* or *that*, and descriptive characterization. What we regard ourselves as *observing* is the object-referent. But, surely, he is quite right in calling attention to the fact that we can talk about our personal experiences of hearing sounds and "seeing" visual fields. According to my own theory of the *how* of perceiving, these guide our objective claims. And I do not think that, in his present mood, Ryle is opposed to this complexity. Language is a flexible instrument and, though it may begin in the framework of *observables*, it can, and has, spread to that of personal experiences, as both everyday usage and Tichenerian psychology shows. The added framework is that of *my* or *your* *having* sensations and feelings. And the personal pronouns refer to percipients which are *public* observables, that is, can be observed by others and by themselves. It will be noted that the term observable here is rightly connected with referential description.

Hampshire is concerned with identification and existence as categorial meanings connected with perception. I think he is quite right in stressing them. The percipient regards himself as existent, that is, as perceiving, ready for action, etc. We are far from the artificial set-up of Descartes which has so muddled philosophers, with its combination of the causal theory with intuitive rationalism. And the percipient regards the object he is referring to, and describing, as equally existent with himself, the percipient. This is normal and normal usage.

But complications enter when Hampshire considers the statement, "That *white spot* which you see on the horizon is a boat". We have called attention to the point that sensations *guide* the perceptual act of reference and the descriptive characterization and thus introduce a functional complexity native to *sense-perception*. It was this situation which, we saw, bothered Moore. It lies back of the familiar two uses of the verb "see". There is, thus, an ambiguity in the use of the term, *white spot*, which must be cleared up. Two people can see the same physical thing in the perceptual sense of see. And both can *have* a *white*, sensory factor guiding their observation. But *white spot*, so taken, cannot be a boat, though the boat can be descriptively *white*. But it is in such situations as these that we easily understand that our *sense-perceptions* are at the mercy of distance and are no more than guided knowledge-claims which are likely to be

veridical. But, in all these cases, we do identify and are concerned with existence, an existence co-ordinate with our own. Hampshire is quite right here; and I always used to make the same point. What science does is to add its technique of measurement and tested theory to this perceptual realism. Pearson and Mach were misled by the old, causal framework which led to the stress upon sensations as termini and not as factors in a pattern pointing, through response, to external things.

I remember how the ingenious Hume, refusing the gambit of representationalism, suggested that we *take* sense-impressions to be things. But one problem was that of accounting for our *notion* of things. To me, it is clear that this *develops* in the setting of perceptual attitude, response and result. That is, we have to adjust ourselves to the things around us. Perceptual realism fits in with the biological setting in a way that subjective sensationalism does not. I seem to remember that Prichard worked this gambit of *taking* sensations to be things. But it will not do. What sensations do is to guide directed responses and to give *cues* for the location and description of things, co-ordinate with ourselves. In this *cognitive context*, they became the *bases* of "broke" and "appeasing", which always have a referential framework. Price seems to be exploring this line.

Let me return now to Ryle. He is, as always, linguistically acute. And he does not want to be lost in the complexities of neurology and psychological, causal theory. And, in one sense, he is right. The epistemologist should reflect upon everyday distinctions and try to do justice to them. And Ryle does bring out the difference in the *framework* of perceiving and of noting sensations and feelings. But I think he is "stumped" because he has not worked out a clear theory of the constitution of perceiving with its reference and description, its external, existential concern and identifications, as against having sensations, tickles, feelings. He does not sufficiently see that, in *perception*, sensations function as guides and not as objects; while, in noting them for their own sakes, they become data of inspection. And he rightly sees that *pains* are not used as *cues* to external description in the way that heat sensations are. After all, the organism has many jobs to do. It has to withdraw, become aware of danger, take note of internal conditions, etc. It is this many-dimensional life which philosophers should keep in mind. But I do think that clearing up the how and the achievement of veridical, referential claims should help to give the essential framework into which personal experiencings can fit both linguistically and as guides. Perceiving is not an impersonal matter. There must be a percipient.

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veridical. But, in all these cases, we do identify and are concerned with existence, an existence co-ordinate with our own. Hampshire is quite right here; and I always used to make the same point. What science does is to add its technique of measurement and tested theory to this perceptual realism. Pearson and Mach were misled by the old, causal framework which led to the stress upon sensations as termini and not as factors in a pattern pointing, through response, to external things.

I remember how the ingenious Hume, refusing the gambit of representationalism, suggested that we *take* sense-impressions to be things. But one problem was that of accounting for our *notion* of things. To me, it is clear that this *develops* in the setting of perceptual attitude, response and result. That is, we have to adjust ourselves to the things around us. Perceptual realism fits in with the biological setting in a way that subjective sensationalism does not. I seem to remember that Prichard worked this gambit of *taking* sensations to be things. But it will not do. What sensations do is to guide directed responses and to give *cues* for the location and description of things, co-ordinate with ourselves. In this *cognitive context*, they became the *bases* of "broke" and "appeasing", which always have a referential framework. Price seems to be exploring this line.

Let me return now to Ryle. He is, as always, linguistically acute. And he does not want to be lost in the complexities of neurology and psychological, causal theory. And, in one sense, he is right. The epistemologist should reflect upon everyday distinctions and try to do justice to them. And Ryle does bring out the difference in the *framework* of perceiving and of noting sensations and feelings. But I think he is "stumped" because he has not worked out a clear theory of the constitution of perceiving with its reference and description, its external, existential concern and identifications, as against having sensations, tickles, feelings. He does not sufficiently see that, in *perception*, sensations function as guides and not as objects; while, in noting them for their own sakes, they become data of inspection. And he rightly sees that *pains* are not used as *cues* to external description in the way that heat sensations are. After all, the organism has many jobs to do. It has to withdraw, become aware of danger, take note of internal conditions, etc. It is this many-dimensional life which philosophers should keep in mind. But I do think that clearing up the how and the achievement of veridical, referential claims should help to give the essential framework into which personal experiencings can fit both linguistically and as guides. Perceiving is not an impersonal matter. There must be a perceiver.

Now I have argued that G. E. Moore did not have this empirical theory of perceiving, as a complex operation in which sensations *guide* the referential application of concepts, and that that is why he was so bothered by the question of the status of sense-data, that is, whether they were part of the surface of the object perceived. And, since he started with sense-data in the traditional way, he had to *prove*, in some affirmative fashion, the existence of the external world. There was the Kantian and Platonic background added for both Moore and Russell, as has recently been brought out by Morton White and Wilfrid Sellars. The critical realist did not try to prove the external world in this fashion. He did not have this kind of artificial problem.

These men were tremendously able. They simply did not have the reorientation necessary. I am convinced that my constant contacts with biologists and experimental, comparative psychologists helped me to work out this new orientation. That was a bit of luck.

My argument, in short, has been that philosophy got off to a bad start in the seventeenth century with the schematism of impact on sense-organs and brain, with terminal, mental states in the soul-mind, somehow given to inspection. The whole schematism is outmoded. The unit is the patterned, sensori-motor complex with interplay between the terms so that sensations guide response and response makes demands upon *discrimination* in sensations, an affair of learning. I could give chapter and verse for admirable, experimental work being done along these lines. A good summary is to be found in C. Judson Herrick's chapter on perception in his recent book, *The Evolution of Human Nature*.¹ I had the opportunity of talking it over with him in its first manuscript form. He has always been an enthusiastic Coghillian, that is, an advocate of the unified action of the organism as a whole as against fixed reflexes. Herrick has always been interested in philosophy. I think this helped him to avoid the retention of dualism, something Sherrington retained. The brain was the organ of behaviour but of a guided behaviour on which intelligence had leverage. He and I agreed on the role of learning as enlarging the stimulus-response circuit. The shift to this as the "open" unit which gave a *setting* to both stimulus and response represented the basic reorientation in the causal theory of perception. Thus we do not start with sense-data as mental units and puzzle as to their evidential connection with inferred objects. We find ourselves instead *right within* a

¹ The University of Texas Press, 1956.

developed, perceptual experience dominated by directed response and linguistically expressed in terms of demonstratives and descriptions. And yet sensory factors still operate in a guiding way and attention can be shifted to them. But the old tradition keeps philosophers still focused on sense-data as *isolated* givens from which, alone, perception starts. As we saw, Russell and the logical positivists—largely mathematicians and logicians—kept at this gambit until, in desperation, Russell, at least, shifted to physics as a point of departure. But, even then, perception tended to vanish into egocentric particulars, somehow connected with the physical world. He had not really reorientated himself on perception. He did not see how perceiving is concerned with material things as referents. That is, this causal approach was still largely traditional. The organic *how* of actual perceiving was not explored.

While the epistemologist has, for his main job, the establishment of a *direct realism* which puts man both actively and cognitively in touch with his world and turns him aside from the vagaries, or muddles, of phenomenism, idealism and scepticism which had empirical orientation (historically conditioned) had encouraged in the past, he must keep in touch with basic shifts in the newer sciences as well as in physics. It is a mistake to stress linguistic usage alone—valuable as that is as a check on absurdities inspired by bad starts—for one must solve problems in co-operation with the sciences.

Now all this leads me up, in conclusion, to the mind-body problem as pivotal. It will be recalled that Perry rejected the subjective and denied that the brain could be the seat of consciousness. He was influenced here by Bergson. He linked his pan-objectivistic presentationalism which was, as we saw, a kind of extroverted Humanism—Russell flirted with this in his neutral monism—with philosophical behaviourism. Now philosophical behaviourism—as against the *methodological behaviourism* of the psychologist, puzzled by introspection and the role of the brain—really reflects an inadequate epistemology. In its linguistic form, it expresses, often, an admirable study of words and sentences as tied in with behaviour. I, myself, have found Ryle extremely suggestive here. But what is the existential status of the concepts which *function* in verbal usage. Of course, they are not objects of inspectional intuition. Yet, surely, these meanings which declare themselves in both patterns of usage and patterns of behavioural response—so closely knit together—arise in that process of learning which the cortex furthers as it connects stimuli with behaviour. So far, so good. But the existential locus of sensory

qualia must also be worked out, if we are to escape dualism. And here we have a kind of knowledge which we can call experiencing or acquaintance with. And my thesis has always been that the *having* sensations connects up with this experiencing.

But where *are* the sensations ? My answer is "in" the brain-mind as a feature of cerebral activity. They are "natural isolates" in that they and not their context are experienced. And they are features of cerebral activity which cannot be reached by external, perceptual knowledge about. The neurologist cannot *inspect* them in the brains he studies for *his* perceiving must be guided by his own sensory data. And the same is true of the behaviourist. The job is, then, that of adequate categorization of a unique situation, uninhibited by presuppositions of a dualistic sort. The Cartesian views of both mind and matter are outworn. Mind is not a substance but a guided activity leading to behaviour. Matter is not mere extension but something capable of high organization and levels of integrative functioning. And knowledge of neither is an affair of intuition. If we split them apart we get a *ghost* in the organism. The mind is the brain functioning (occurently and dispositionally); and the self emerges as the focus of directional activities. Here we have a development of that *guided causality* which we have already noted in referential perception.

I think philosophy has a job of clarification to do here supplementary to that which the biological and psychological sciences are undertaking with no mean success. It is a pity that it has so long been thwarted by retention of antiquated, causal theories, such as those which arose in the seventeenth century, which led to the taking of sensations as terminal units. The actual *unit*, as we have noted, is sensori-motor with centrally aroused processes working out new connections and giving correlated meaning to both. But the *how* of perceiving only opens the way for the epistemologist to appreciate the nature of the directed knowledge-claim which, for philosopher and scientist, is the culmination of guided perception. The other animals respond in guided ways and, often, pretty intelligent ones. Man, the conceptualist and linguist, *lifts* perceiving to the level of demonstrative and descriptive terms, in so doing arriving at explicit knowing.

I have done little more here than to suggest a framework for philosophy, which I have found illuminating. The keenness and great ability of the present generation is, manifestly, so great that, given the right perspective, one can expect a remarkably

constructive period along empirical lines. All sorts of analyses are in reserve ready to be applied.

Looking backward, I am of the opinion that the critical realists had the better hunch in their emphasis upon reference as against inference. It contained the possibility of escape from both presentationalism and representationalism. Perceiving is not an affair of concern for sensory data as such. The *percipient* is concerned with the things around him and uses his sensations as guides for directed location and characterization. This is a direct realism for the object is co-ordinate with the percipient organism. We *have* sensations but *perceive* things. It is an achievement based on the controlled growth of applicable concepts tied in with language and response. It is not an intuition. Common sense is right in accepting it but has no clear idea of the nature of the deed. The tragedy of philosophical reflection was that it got off to a wrong start because of historical assumptions. What the critical realist suggested was a reorientation. No doubt he fumbled the ball.¹ So much had to be worked out. But I cannot get over the feeling that logical positivism was *not* very original, that it did not contain a basic reorientation in touch with biology and psychology. How could this be expected from men who were primarily logicians and mathematicians? But they have worked from proposal to proposal with a dedication which is, itself, praiseworthy. Could philosophy be taken by storm, Carnap would have climbed the ladder. The note I would end on is that of encouragement to the whole maligned tribe of epistemologists, axiologists and metaphysicians. If the framework I have outlined is justified, we can *know* our world, *appraise* its actualities and possibilities, and *decipher* the categorial constitution of what is. What if it does turn out to be mother nature?

To sum up: Critical realism is a direct realism after the manner of naïve realism. But it explains perceiving as a cognitive achievement by studying its mechanism and its development in the framework of S-C-R. In this context, it can account for sense-data theories and "appearances". After all, it is sense-perception. G. E. Moore did not dig deeply enough into the mechanism of perceiving and Russell always tended to substitute "percepts" for perceiving. We have referential claims and *not* cognitive relations and reference is a development of response.

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¹ Or, more accurately, new, and rather clamorous, movements secured the limelight. Philosophy develops in rather zigzag ways. It looks as though the "how" and achievement of sense-perception will again be studied apart from slogans.

III.—OTHER DATES

By RONALD J. BUTLER

I

BERTRAND RUSSELL once remarked that there is nothing logically impossible in the hypothesis that the world was created or sprang into being five minutes ago, exactly as it then was, complete with the memories and the monuments and the little scraps of paper. On his view, there is nothing logically impossible in this, because there is no logical connection between events occurring at different times; and therefore nothing happening now or likely to happen in the future is incompatible with any past whatsoever, including the hypothesis that the world (or the universe) began precisely five minutes ago. Like all sceptical hypotheses, Russell went on, this hypothesis is logically tenable but uninteresting; and there he left the matter.¹

But on the contrary, the notion that the world came into being five minutes ago, complete with a population which "remembered" a wholly unreal past, is fascinating—but untenable. It was Edmund Gosse's father, I believe, who bolstered his fundamentalist views with the ingenious theory that God had created the world in 4004 B.C., with its fossils and lava and other evidences of a distant past, for the sole purpose of trying men's faith; and in principle Russell's suggestion is analogous. I suppose his insistence that there is no logical connection between events occurring at different times is correct, in that from a statement reporting one occurrence we cannot deduce without additional information a statement reporting another occurrence. But in a sense of "logic" currently used at Oxford, this hypothesis is so untenable logically, that when we write of a population "remembering" an unreal past, we enclose the "remembering" in shudder-quotes, as a matter of course. In this sense of "logic", the hypothesis that everything before the last five minutes did not really happen plays havoc with the logic of words which refer beyond the last five minutes. If a person says he remembers something which we later prove could not possibly have happened, we tell him he did not remember it, he must have imagined it. I am tempted to say that because remembering is contrasted with imagining in this way, the notion of remembering a wholly unreal past is queer. Very queer. But it is not queer precisely

¹ B. A. W. Russell, *The Analysis of Mind* (London, 1921), pp. 159-160.

because of this contrast with imagining, for it would be equally queer to say that such a population "imagined" their past. Have they not all the grounds for *claiming* to remember: memories and monuments and little scraps of paper? The queerness lies rather in the fact that any particular occasion they would lack, by our standards, the criteria for discerning what they imagined from what they remembered.

We enclose the "remembering" in shudder-quotes because their criteria do not conform to ours. This does not mean that they have no criteria of their own. If, for example, their memories of what had happened were not corroborated by their other evidences for the past, they could draw a distinction analogous to our own: they could say that when they seemed to remember they must only have been imagining. Although such a population might have usages analogous to our more genuine remembering and imaginings, their uses would differ in the Pickwickian respect that whatever they remembered having happened prior to five minutes ago would be unreal, *ex hypothesi*. But, it might be objected, although one cannot remember an unreal past, in any ordinary sense of "remember", at least one can contemplate the possibility of a population seeming to remember a past that never was, as if it were the real past, honouring it with Thanksgiving Day and Christmas and all the rest, although there never were thanksgiving fathers and there never was a Christ, and indeed there never was a past, until five minutes ago. Suppose we could observe a historian at work in this population: he could proceed with precisely the same criteria of scholarship as do our historians. But every time he commented on something that had happened in the past, we with our superior knowledge would think,

Of course, that is what the evidence, coupled with his awareness of things remembered, obliges him to believe about the past; but he has no real knowledge of the past. There was no real past—until five minutes ago.

Here we have unfolding before us the problem of other dates, analogous in many respects to the problem of other minds. An approximately parallel statement of the latter problem might be,

Of course, that is what his overt behaviour, coupled with an awareness of my own inner state, obliges me to believe about his mind; but I have no real knowledge of other minds. Perhaps even there are no other minds.

That such a population could not remember their past in any ordinary sense of "remember" does nothing in itself to disprove the hypothesis. The disparity between ordinary usage and a

suggested hypothesis merely serves to show that if the hypothesis were accepted, we would need to modify ordinary usage—something we are doing all the time anyhow. Fortunately, the illogical character of Russell's hypothesis can be demonstrated by a more rigorous method.

Scepticism concerning the reality of the past is a queer kind of philosophical doubt, because once the hypothesis is formulated there is nothing which members of the population might do either to confirm it or disprove it. How can we be sure that *all* populations, including our own, do not use "remembering" and "imagining" in the Pickwickian sense? How do we know that we ourselves are not caught up in this sceptical flight of fancy? The answer, I think, is conclusive. With no conceivable way of establishing its truth or falsehood, one is inclined to balk at admitting it as a genuine hypothesis at all: especially as once this kind of scepticism is admitted, even *in a minor degree*, there is no stopping even at *this* hypothesis. If we are not sure about the sixth moment ago, how can we be sure of the fifth?—or the fourth?—or the third? There are no half-way houses on the highroad to extreme scepticism: one step, and in the face of ever-encroaching questions about how we *really* know, there is no stopping short of knowing only the present instant. All these doubts, of a kind that afford no conceivable method of proof or disproof, lead inevitably to instantaneous solipsism; and this in itself would seem to constitute a conclusive *reductio* of such doubts *ad absurdum*.

II

I have developed some of the implications of Russell's suggestion that there is nothing logically contrabrand in the hypothesis that the world came into being five minutes ago because, as a result of mistaking the role played by evidence in our knowledge of the past, a number of contemporary philosophers of history and practising historians are in danger of having this queer kind of philosophical doubt laid at their doorsteps. The tendency I am referring to is epitomised in a statement of Professor G. C. Field:

When the historian says, This happened, that is always an abbreviated phrase for, The available evidence points to this having happened. In an extreme case, I suppose, the assertion that this happened, so understood, might be true, though, in fact, this never did happen.¹

¹ G. C. Field, "Some Problems of the Philosophy of History", *Proceedings of the British Academy*, xxiv (1938), 70. Cf. A. J. Ayer, *Language, Truth and Logic* (2nd ed., London, 1946), pp. 101-102 and pp. 18-19, also C. I. Lewis, *Mind and the World-order* (New York, 1929), p. 151.

Field may have meant, in the context of this remark, that the historian is never able to make statements about the past which can be warranted without reference to historical evidence; and with this nobody would disagree. But if all our statements about the past *were* statements about evidence, as Field, Lewis and Ayer all seem to suggest, then it would follow on two counts that we have no *real* knowledge of the past. First, because no matter how well authenticated the evidence may seem, there is always the possibility of conflicting evidence turning up. Secondly, because in the last resort everything which can count as historical evidence—memories and monuments and little scraps of paper—is contemporary with the historian who is reconstructing the past: there is this much justification for Croce's dictum, "All history is contemporary history". In view of the paradox that our knowledge of the past derives entirely from our knowledge of the present, Professor Michael Oakshott maintains that the historian's aim is to describe "Not what really happened, but what the evidence obliges us to believe":¹ and although it is doubtless a misreading, it would sometimes appear from his writings that Oakshott would have no qualms at all over the hypothesis that the world was created precisely five minutes ago. At this point Field pulls Oakshott up by declaring that the historian's aim is to describe what the evidence obliges us to believe *about what really happened*: "for unless we assume that something really happened, the evidence doesn't oblige us to believe anything." In so far as Field is correcting Oakshott, the correction is surely a valid one.

The force of Field's view cannot be denied. Any historical statement we make is based upon evidence; and there is always the possibility—albeit in some cases a very weak kind of possibility—that counter-evidence might turn up. This does seem to commit us to the view that statements about the past are at best only probable, even if sometimes the degree of probability is very high. On this view, because there are no historical statements which could not be overturned, at least in principle, therefore there are no historical statements which are absolutely certain. From this it is concluded that the *most* the historian can say is, "The evidence obliges us to believe that this happened" (or, more guardedly, "The evidence points to this having happened" or "The evidence warrants a belief in this having happened"): and any contraction to "This happened" is elliptical. It is

¹ Quoted by G. C. Field, *ibid.* Cf. M. Oakshott, *Experience and its Modes* (Cambridge, 1933), pp. 107–108.

important to emphasise that the force of this view rests entirely on the proposition,

There are no absolutely certain historical statements, because there are none which could not in principle be falsified by further evidence.

This proposition, from which Field's theory derives its entire force, is indisputable; so that any denial of Field's attempt to assimilate statements of historical fact to statements about historical evidence must accommodate this proposition too. Let us examine the strength of the kind of possibility involved: it is, I think, what philosophers have sometimes, perhaps misleadingly, called logical possibility. In the case of well-authenticated judgements, its strength is generally admitted to be weak; and the more conclusive the evidence the weaker the possibility seems to become. Keeping this in mind, it is relevant that when we say that any statement of historical fact is prone to being falsified by further evidence, we are duplicating an already weak kind of possibility. First we admit that if further evidence *were* to turn up, then possibly a statement of historical fact would be falsified; and then we admit that further evidence *may* turn up, which leads to the conclusion that possibly a statement of historical fact *may* be falsified. If p be a statement of historical evidence, and q be a statement of historical fact, then the argument, in Polish symbolism, becomes

$CCpMNqCMpMMNq$

which we might read off literally as

"If p implies possibly not- q , then possibly p implies possibly-possibly not- q ."

or in better English,

"If p implies that not- q is possible, then the possibility of p implies the possibility that not- q is possible."

In this duplicated sense, it remains possible that any statement of historical fact may be overturned by a statement about historical evidence.

My denial of Field's assimilation of statements of historical fact to statements about evidence involves the assumption that *what counts as good evidence has a temporal character*. Ranke was prepared to accept the testimony of eye-witnesses,¹ whereas the current tendency is to place more and more emphasis upon the evidence of "witnesses in spite of themselves".² There is a

¹ T. H. Von Laue, *Leopold Ranke—the Formative Years* (Princeton, 1950), p. 120.

² M. Bloch, *Apologie pour l'Histoire, au Metier d'Historien* (Paris, 1949), pp. 24 ff.

tendency to say that what counts as good evidence has a temporal character, while historical facts in themselves are non-temporal.¹ While there is a contrast between truth and evidence in this respect, this way of putting the matter is an over-simplification. For reasons argued elsewhere, no harm seems to arise from speaking of propositions becoming true in the course of time, whereas the assumption that future contingent propositions already have a truth-value is metaphysically suspect.² The distinction which concerns us here is rather that once something has happened it has happened irrevocably, whereas there is nothing irrevocable in the admission of evidence.

This distinction between statements of historical fact and statements about evidence can be sharpened by a *reductio ad absurdum* of any attempt to assimilate them. If "This happened" were always elliptical for "The available evidence points to this having happened", then we could only dispute the historian's statement by showing that, when he made it, he failed to take into account all the data which he could reasonably have been expected to recognise as evidence. But the evidence which a historian like Gibbon recognised, and the emphasis which he placed upon it, are to some extent matters of his personal evaluation, true for Gibbon and not subject to the criteria of critics. This being so, if statements of historical fact are to be reduced to statements about evidence, then every statement in the *Decline and Fall* which seems to be about something that happened is really about evidence, to be tacitly prefaced with some such phrase as "In Gibbon's judgement...". No subsequent evidence could invalidate such a statement apart from evidence that his opinion had not been accurately recorded.

Furthermore, even if verdicts about the nature of the evidence were not to some extent matters of personal evaluation, it remains true that the historian of the Crusades will agree with Gibbon not, save *per accidens*, on whether Gibbon correctly evaluated the evidence available *then*, but with whether his account of what happened accords with what the present-day historian says happened. It is true that whenever Gibbon cites his sources, and he not infrequently does this, the present-day historian may want to query the use to which the sources have been put. But Gibbon does not always cite his sources, and it would be a task of considerable difficulty to establish precisely what evidence was

¹ Cf. R. Carnap, "Truth and Confirmation", *Readings in Philosophical Analysis*, ed. H. Feigl and W. Sellars (New York, 1949), p. 119.

² Cf. "Aristotle's Sea Fight and Three-valued Logic", *Philosophical Review*, lxiv (1955), 270 ff.

available to him. This is clear from the fact that a statement of the form

The evidence available now concerning what evidence was available to Gibbon when he said ' x happened ' (meaning that the available evidence then pointed to x having happened) points to such-and-such a conclusion.

could never be used to correct Gibbon's statement that in fact x happened. Gibbon's evaluation of the evidence he examined, in all its detail, as contrasted with his evaluation of the situation about which he was writing, is of interest for the most part to his biographers rather than his fellow-historians.

The cumulative effect of these last two arguments is that if our concepts of truth and evidence are not carefully distinguished, and if an attempt is thereby made to reduce statements of historical fact to statements about evidence, then the *Decline and Fall* would have to be taken on trust as eternally true and incorrigible. But in practice it is assumed that so long as the judgements of a dead historian continue to have bearing upon relevant problems, the debate between the dead and the living can go on. Perhaps Field would have replied, as did Mannheim, that when we say a statement of Gibbon's is false, what we really mean is that Gibbon's statement, which referred to evidence available then, has been *superseded* by later statements made after the discovery of new evidence. Gibbon's statements would not then be wrong, but simply out of date. The objections are numerous, however, to any theory which attempts to replace historical truth by historical fashion, what historically was the case by what is now historically acceptable. Suffice it to say here that historians in practice assume they can do more than merely show Gibbon to be out of date. They assume they can discuss the extent to which he was wrong, even to the point of contradicting him on occasion. If it were merely a matter of whether his statements conformed to current fashion, there might be point in abusing or lauding his work: there would be no point at all in wanting to contradict him.

Some of the difficulties which arise from refusing to draw this distinction are contained in the statement, quoted above, which Field made immediately after he had identified statements of historical fact with statements about evidence :

In an extreme case, I suppose, the assertion that this happened, *so understood*, might be true, though in fact this never did happen (*my italics*).

To begin with, the last clause, "... though in fact this never did happen", is something which Field cannot *say*. On his own

analysis, " x did not happen" is elliptical for "the evidence indicates that x did not happen". The statement, " x happened" might be true, though in fact x never did happen," is therefore a manifest contradiction, to the effect that the evidence indicates that x happened, though in fact the evidence indicates that x did not happen. By identifying statements of evidence with statements of truth, Field has denied himself any means of saying consistently that what seems to be true may run counter to the facts.

Let us suppose that Field intended to remark not only that the evidence might point to something having happened which never did happen, but also that *future* evidence might turn up to show that it never did happen. Such extreme cases are not unknown: James Ussher, Primate of Armagh, suggested in 1617 that the world was created in 4004 B.C. Subsequent geological evidence led to the rejection of this belief, although in the accompanying controversy it was tenaciously maintained by some people on irrational grounds. But men of Ussher's time had no geological evidence at all, and the question of how long it takes to form rocks and deposit strata had not occurred to them. They unearthed no remains of human life from a stratigraphical level lower than that of societies which, in their written histories, engendered the belief that they were the first created, nor from a level so low that the question of how long it had taken to deposit the earth over them need arise. They therefore tackled the problem from the point of view of the only evidence they had, which was written evidence. There were Hebrew genealogies, which provided a count of the years from the creation of Adam in terms of the lifetimes of the patriarchs, the Greek system of dating by Olympiads, and accounts of the dynasties of Egypt and Syria bearing the names of Manethon and Berosus. These had certain features in common: for example, they tended to encourage the idea that man and the earth were created at the same, not too remote, moment; and several of them contained references to a Flood. But they did not all tell complete stories, or for that matter, the same story. There therefore existed the science of chronology, which set out to criticise and harmonise these various systems of dating and the stories they told, usually, but not invariably, on the assumption that the Hebrew chronology was inspired, and the others must be made to conform to it.¹ Contemporary scholars accepted Ussher's 4004 B.C. as the most plausible and accurate synthesis of the evidence then known to

¹ P. Hazard, *La Crise de la Conscience Européenne*, i (Paris, 1935), 50 ff.

them. Even if his conclusions were incorrect, there was nothing absurd in his endeavour, for he worked critically on the evidence then available in accordance with his presupposed rules of procedure, which included the pre-scientific belief that part of the evidence was divinely inspired. There is a sense of "true" in which this conclusion was *true* for Archbishop Ussher; but, as we have seen, this idiom of being true for somebody but not necessarily true in fact is inadequate. Such examples point rather to the distinction between an honest judgement which can be believed as if it were true, and a judgement which is in fact true. The assertion that x happened might be a plausible verdict drawn from an honest examination of the evidence; but in the light of later evidence it might well turn out to be incorrect.

III

Considerable caution is necessary in presenting the denial of Field's thesis, that when the historian asserts that x happened he is *not* always asserting that the evidence obliges him to believe that x happened. Difficulties arise, for example, if we attempt Field's substitutions in the context of statements expressing beliefs. Is it plausible, one might ask, to assimilate, "*I believe x happened*" to "*I believe the evidence obliges us to believe x happened*"? It would be generally granted that a statement purporting to assert a fact, such as "The apostle Peter *became* the first Bishop of Rome", is different from a statement of belief, such as "*I believe that the apostle Peter became the first Bishop of Rome*". Yet if the suggested substitutions are correct, then the first statement is not about Peter, but about evidence concerning Peter; and the contrast between the two statements is maintained by making the second a higher order belief, to the effect that *I-believe-that-the-evidence-obliges-us-to-believe that Peter became the first Bishop of Rome*. While it is true that the historian is concerned, on occasion, with beliefs about beliefs, nothing is gained by saying that in asserting what he believes about a past event, he is really giving expression to a belief about a belief. If, for example, the evidence obliges the historian to believe that Charles I was executed because his policy was unfavourable to the minority which happened to be in the ascendant, then he believes and asserts that Charles I was beheaded for that reason. He does not assert that *he-believes-that-the-evidence-obliges-him-to-believe that Charles I was beheaded because...* It is the conclusion itself, rather than how he arrived at the conclusion after examining certain evidence, which the historian asserts and believes. If

he is wise, however, he will assert his conclusion tentatively and modestly, because invalidating evidence may well turn up. After all, the historian believes that such-and-such happened because he also believes and accepts certain evidence which points in that direction, and if this belief about the evidence is called into doubt, then the belief that such-and-such happened should logically be called into doubt too. All first-order beliefs are connected in this way with other beliefs—in the case of history, with beliefs about evidence. Now, it would be misleading, besides being pointless, to say that *because* all our historical beliefs are thus warranted by their connection with beliefs about available evidence, *therefore* our historical beliefs are not about what they appear to be about, but instead are really second-order beliefs—beliefs about what the evidence obliges us to believe.

It may well be that those who assimilate statements of historical fact to statements about historical evidence would not accept this analysis leading to higher order beliefs. The alternative open to them is to identify "*x* happened" with "I believe that *x* happened": it follows that for them "I believe *x* happened" is identical with "The evidence (in my opinion) points to *x* having happened" (or "obliges us to believe that *x* happened"). To some extent this latter identification is supported by ordinary usage. Certainly the historian often has cause to say what the evidence obliges us to believe, especially when a conflict of evidence leads him to query a previous judgement. For example, the popular account that Charles I was beheaded because he had proved himself a tyrannical leader might be rejected upon discovering that the majority of his subjects, far from thinking him tyrannical, had little idea why he was executed. On the contrary, *the available evidence obliges us to believe* that he was put to death because his presence was unfavourable to a minority which happened to be in the ascendant. At other times the historian will say, "Perhaps the evidence points to such-and-such having happened" when in fact the evidence is so scant that any conclusion at all is conjectural. For instance, did the apostle Peter become the first Bishop of Rome or did he journey on a mission into Asia Minor with his wife? On weighing the varying traditions, one might decide that *the evidence points to this or that having happened*; but in either case the verdict would be inconclusive. In this example, as in the example of Charles I, the appeal to what the evidence obliges us to believe (or points to having happened) is a persuasive appeal, an invitation to share the historian's belief.

Not all historical statements are mere invitations to share the historian's belief; for if they were we would have no desire to

~~issue~~ the invitation. The value of observing the ordinary use here lies in the fact that it focuses our attention upon the historian's desire *sometimes* to pronounce beliefs about what happened in the face of conflicting or inconclusive evidence, and if need be to persuade others that his beliefs are the right ones. But nothing at all is achieved by showing that a suggested theory flies in the face of an ordinary use—unless it precludes us from saying what we would ordinarily want to say. This merely serves to remind us that if Field's theory won wide acclaim, we would need another idiom to express a current distinction.

If a person says he believes something to be the case, not in the strong credal sense of believing in the Holy Ghost, but in the opining sense of believing in the face of insufficient or conflicting evidence, then his statement of belief is his opinion of what the evidence points to. Because what the evidence points to is what the historian believes, any analysis which leads to the form "I believe-that-the-evidence-obliges-us-to-believe" is redundant. In other words, the distinction between assertions of fact and assertions of belief cannot be made by means of Field's substitution: *no* distinction has been made.

There is another use of "what the evidence obliges us to believe" from which Field's theory derives considerable force; but I am by no means convinced that it is more primary than the use we have already discussed. It is sometimes true that verdicts about the strength of the evidence are asserted as verdicts about what happened. Suppose a jury were to decide *in camera* that the evidence overwhelmingly pointed to the accused's guilt. If at this stage of the court's proceedings the judge learned the jury's verdict about the strength of the evidence, he would be entitled to infer that their verdict concerning the accused would be "Guilty!" Nevertheless, the formalities of the court demand that the jury present, not their verdict concerning the strength of the evidence, but rather their judgement concerning the guilt of the accused; and it is this verdict which the judge endorses or fails to endorse. But in the case of the historian the formalities of the court are dispensed with. Sometimes he may merely say, "The evidence overwhelmingly points to *x* having happened"; and this is taken as license to infer, without further ado, that he believes *x* happened. This does not mean, however, that verdicts concerning the strength of the evidence *are* verdicts concerning what happened; although it serves to indicate how narrow is sometimes the logical gap between statements of evidence and statements of fact. On occasion these are so intimately related that an assertion concerning what the evidence overwhelmingly

indicates is tantamount to asserting what was the case—*tantamount to but not the same as*.

What is the relation between these two kinds of statement? The historian believes that in fact Charles I was executed because he also believes that a powerful minority to whom the king's policy was unfavourable was in the ascendant, and he believes that Charles's execution was in their interests because he accepts a whole host of other evidence about Cromwell's party during this period. He accepts these judgements because too much of the historical world in which he believes is implicated and would be carried down if they fell. Now, when one passes an historical judgement, one cannot have in mind all the other beliefs which are implicated; but that does not mean that one cannot have some of them in mind. Indeed, epistemologists seem to have neglected the point that one can have simultaneously quite different, although nevertheless related beliefs *in mind*. It is not at all implausible that a historian should believe something happened and also believe, at another level as it were, that the evidence points to this having happened. Furthermore, it may well be that even when these two different beliefs are not *before the mind*, the latter is nonetheless implicated in the former, in such a way that if the historian's judgement that something had happened were challenged, he would immediately justify his belief by drawing attention to the relevant evidence. Any recourse to talk about levels or orders of belief, however, is misleading if we picture different levels of geological strata, or the higher orders of an algebraic variable. What is essential is that although the one kind of judgement may well be implicated in the other, *nevertheless they are two distinct kinds of judgement*. The importance of refusing to assimilate "This happened" to any variant of "It seems as if this happened" lies in the logical gap which separates any collection of statements of the form "It seems as if . . ." from the conclusions to which they lead, "It is the case that . . .". This gap can never be bridged by assembling more and more statements about how things seem. As G. J. Warnock recently put it,

... there is an essential logical gap between discussing evidence and pronouncing verdicts—a difference which cannot be abolished by any amount, however vast, of piling up evidence, however conclusive.¹

The difference between these two kinds of statements is that whenever we say that something *seems* to be the case, we are always attempting to meet or accommodate a doubt; but when

¹ G. J. Warnock, *Berkeley*, (London, 1953), p. 186.

we say that something is the case, we are either concerned to dispel all doubt, or else in the context we have no doubts to dispel. The gap between verdicts concerning evidence and verdicts concerning past events cannot be bridged by deductive inference ; and this is the sole justification of Charles Beard's statement that written history is an act of faith.

IV

We have already emphasised that the reason why one is tempted to substitute "The available evidence points to this having happened" (or "obliges us to believe that this happened") for "This happened" is that there are no absolutely certain historical statements, because *there are none which could not in principle be falsified by further evidence*. Can this proposition be reconciled with the assertion that statements of historical fact are asserted *as if* they were certain, by an act of "faith"? We have already stressed that if a historian is wise, he will pronounce his verdicts tentatively and modestly, precisely because invalidating evidence may well turn up. But any proposition of the form, "This happened, but I may be wrong", savours of paradox. When I say that I may be wrong about whether Stalin was assassinated in the Kremlin in 1953, I am expressing doubts as to whether I have sufficient grounds for asserting that Stalin was assassinated then. Now let us compare

- (i) "I believe that Stalin was assassinated in the Kremlin in 1953, but I may be wrong", with
- (ii) "I know that Stalin was assassinated in the Kremlin in 1953, but I may be wrong".

Clearly the second of these statements is absurd, and for this reason: that when I claim to know this of Stalin, part of my claim is that my grounds for asserting and believing it are sufficient for asserting and believing it with confidence, while the qualification "... but I may be wrong" announces that I lack this confidence. But the other statement is not absurd in this way: "I believe that Stalin was assassinated in the Kremlin in 1953" indicates that I at least have sufficient grounds for believing it, but it does not indicate that I have sufficient grounds for believing it *with confidence*; and indeed the qualification "... but I may be wrong" explicitly excludes this possibility. It indicates that my grounds for believing are limited rather than conclusive. Again, since "... but I may be wrong" is used to limit a claim about the evidence which has already been staked, it cannot properly be coupled to a statement in which no such claim has been made. For this reason, the proposition

(iii) "Stalin was assassinated in the Kremlin in 1953, but I may be wrong", is logically improper, unless there is a clear contextual indication of the kind of claim the speaker is making.

Let *p* stand for "Stalin was assassinated in the Kremlin in 1953". There is a tendency to say that, whereas *p* is *about* Stalin, in the Kremlin in 1953, the propositions "I know that *p*", "I believe that *p*", and "I may be wrong about *p*" are *about* my attitude to *p*, in Princeton in 1957. But this is a complete mistake, because it suggests that the qualification is about me in the same sense as *p* is about Stalin. There is a further tendency to say that *p* is sometimes short for "I know that *p*" or "I believe that *p*". But if "about" were being used in the same sense on both the above occasions, this would mean that the speaker always used *p* to make a claim about himself rather than about Stalin. The mistake lies in regarding statements beginning with "I know", "I believe", or "I may be wrong" as psychological descriptions, when in fact they do not *describe* the speaker's attitude, but *express* it. The statement, "I believe that Stalin was assassinated in the Kremlin in 1953" has an autobiographical aspect, but that does not make it purely autobiographical: it tells us something about the speaker, but this is not what it is primarily about. Suppose I utter the statement on one occasion only, to a mixed audience. The attitude expressed might be of special interest to the psychologist, a biographer might jot down the kind of belief expressed, a political inference might be drawn from the fact that I assert this statement categorically, a phonetician might busy himself with the intonation and enunciation, while for their various reasons the statistician and the journalist might concern themselves with the content of the belief. One statement, one proposition, with several different aspects, answering several different questions. . . . When the historian hears my statement, what interests him is my belief *that* Stalin *was* assassinated for such and such reasons. . . . He is interested not only in *p*, but also in the kind of authority with which I assert *p*. Expressions of attitude are to be contrasted with descriptions of attitude, such as "When *I* think of Stalin, I am overcome with dutiful affection" or "When *I* think of Stalin my blood curdles". Unlike these, expressions of attitude are neither psychological nor autobiographical descriptions, but are simply devices for providing a contextual setting for the proposition by indicating a change of gears from what has gone before, or by otherwise indicating a claim about evidence. If a person believes something and wishes to express his belief, he will do so either by making a truth-claim

explicitly or by manifesting behaviour from which we can infer a truth-claim. Mr. Urmson, from whom the above analysis derives,¹ reserves the parenthetical use for explicit truth-claims. I see no reason, however, for regarding the parenthetical use as anything more than a stylistic device, used often but not exclusively for *oratio obliqua*. A historian may never tell me directly that he believes Stalin was assassinated in the Kremlin in 1953, and yet I may be able to infer from his other statements either

"He believes that Stalin was assassinated in the Kremlin in 1953",

or

"Stalin, he believes, was assassinated in the Kremlin in 1953",

there being no more than a stylistic distinction between the two. Where then is the recondite philosophical distinction cloaked by the conjugation of parenthetical verbs?

If we say that the faulty coupling of (iii) is due to the fact that *p* is about Stalin, whereas the qualification is about the speaker's attitude, we are still tending to regard professions of belief as psychological descriptions. We are still impaled upon one horn of the old dilemma, that either professions of belief and claims to knowledge describe mental occurrences, or else they describe tendencies to occurrences. On the other hand, this dilemma is avoided if we deny outright that professions of belief are psychological descriptions at all, either of mental occurrences or of dispositions, and assert instead that the whole of proposition (iii) is about Stalin. In this case the expression of belief that *p* is the case is the indication of a special kind of truth-claim about *p*. Generalising, we may say that the expression of an attitude towards something which could be either dogmatically asserted or denied, or tentatively opined or believed, is an indication of the kind of truth-claim we would make about the proposition in question. In the case of (iii), the faulty coupling arises because the qualification "... but I may be wrong" refers to a truth-claim already indicated, whereas no such truth-claim has been indicated.

V

This insistence upon the right to believe something tentatively, whilst fully recognising that one might be wrong, enables us on

¹ J. O. Urmson, "Parenthetical Verbs", *MIND*, lxi (N.S. 1952), 480 ff.

occasion to suspend our judgement, or to be inclined not to believe, without in any way becoming involved in that kind of doubting which leads inevitably to extreme scepticism, to suspending our judgement *all the time*. Hume, at the end of the first *Enquiry*, remarks that "there is a degree of doubt, and caution, and modesty, which, in all kinds of scrutiny and decision, ought for ever to accompany a just reasoner". But he presents this hallmark of sound scholarship as a species of that extreme scepticism, "a small tincture of Pyrrhonism", which we have observed in Section I to be self-refuting even in small doses. This is a mistake. The kind of question which Hume had in mind when he talked about mitigated scepticism, such as "How can I be sure, in the face of conflicting evidence, that the apostle Peter became the first Bishop of Rome?" is of quite a different stamp from the Pyrrhonist's question, "How can I *really* know anything about the apostle Peter, when present evidence is all I have to go on, and I can only accept what the evidence obliges me to believe?" In fact, as we have already seen in Field's correction of Oakeshott, the evidence does not *oblige* me to believe anything. This use of "oblige" has little to recommend it. Even in a valid argument, deductive or inductive, I am not obliged to accept the conclusion, unless my aim is to follow the rules of rational inference. I could equally well decide, on the recommendation of those concerned with the power of positive thinking, to think only happy thoughts. In historical investigations, it is *my* evaluation of the evidence in accordance with accepted rules of procedure which leads me to make certain decisions about what happened. The strength of the truth-claim with which I utter the statement will in a large measure be determined by the kind of evidence which I could bring forward if pressed. If the evidence is so conflicting that decisions concerning what happened are out of the question, then I tend to defer passing a verdict altogether. But the idiom of Field's substitution is a means of *always* deferring verdicts, because the stock uses for pronouncing verdicts persuasively in cases of conflicting or inadequate evidence—"In my judgement . . .", "The evidence obliges me to believe that . . .", "The evidence points to . . ."—are all of them given a new use, which is said to be the *only* use available to the historian in asserting historically. This plays right into the hands of the sceptic who wants to ask, "How can you really know anything about the apostle Peter?—or Charles I?—or about Stalin's death in 1953 when all you have at your disposal is present evidence?"

The realisation that what counts as good evidence is continually liable to change, and that what I am prepared to recognise as evidence is dependent upon my own evaluation, resolves several difficulties concerning the relation of statements about evidence to statements of historical fact. For example, the statement "This happened, but evidence may exist now to show that this did not happen" rests on the confusion *between* evidence, which becomes evidence in being recognised, and *data* which might become evidence if its relevance were recognised. With this distinction in mind, there is nothing paradoxical at all in saying, "On the basis of the evidence available now, this happened, but evidence may turn up *in the future* to show that this did not happen".

A theory such as Field's is liable to conjure up a misleading picture of how we know past events. It suggests that between the historian and the past falls a curtain of evidence through which we cannot peer. Any statement about a past event is reduced thereby to a statement about what the curtain of evidence warrants us to assert. I hope to have shown that the essential logical gap between statements about evidence and statements of historical fact is such that this picture of the historian's activity is false. My most conclusive argument is that no attempt to reduce a statement about a past event to a statement about evidence can avoid becoming entangled in the mesh of excessive scepticism. Conversely, no statement about evidence is, on its own, a logical substitute for a statement about a past event—although in some contexts to utter the one may be tantamount to uttering the other. Therefore, when we claim to know a past event, we are doing something different from merely evaluating the evidence. On such occasions we do not think of the evidence as an impenetrable curtain: we claim to be looking through the fabric, and beyond. This in itself is important preliminary information about our concepts of the past—preliminary, because we have yet to analyse "looking through the fabric, and beyond".

Precisely the same arguments apply when we claim to know what is going to happen, on the basis of so-called conclusive evidence, or what is likely to happen, on the basis of less conclusive evidence. In Christopher Fry's *The Dark Is Light Enough* one of the characters prepares us for the person of the Countess Rosmarin by relating an anecdote which serves to emphasise how punctiliously she had hitherto received her household on Thursdays:

'Good God', she said, 'I think the monkey
Means to be born on Thursday evening'.
But she received us all at seven o'clock,
And at nine, when Gyorki was saying, as usual
That there is no clear truth except the present
Which alters as we grasp it,
She bowed to us in the doorway, and said
'We must freely admit the future', and withdrew
To give birth to Stefan.

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IV.—THE IDENTITY OF A WORK OF ART

BY JOSEPH MARGOLIS

WE speak ordinarily of a translation of a poem. But we are much exercised in explaining whether the translation is the same poem as the poem before translation—and if so, what the difference is—or whether the translation is a different poem—and if so, what is its relation to the other—or whether the translation and original stand in some other relationship that is obscured by our usage. It is also not clear whether the problem of translation occurs in counterpart forms in arts other than literary. What for instance is the relationship between the text of a play and the mounting of a play that employs that text, and what is the relationship between two mountings that are interpretatively different and that employ a common text, or that employ different versions or abridgements of a common text? Or are there even further complications resulting for arts that are not merely literary, as drama and opera, in using translations of an original text? Are there comparable problems in the so-called performing arts, even where the medium of the art does not serve the same formalized referential role as does language in literary art? How is a change of key in a performance related to an original score of music? What is the relation between two performances of a score, one in the original key or rhythm, the other altered? What is the relation between two performances of a score, one with the instruments originally indicated, the other with altered instruments? Are there important properties common to the translating of a poem and to the transcribed performance of a musical composition? What of the art of the dance, in which only incomplete scoring has been recently achieved? Are two performances by the same artist of a dance with a given title, ostensibly intended to be the same, really the same dance? What of performances by different dancers? What is the relation of a reproduction of a painting to the original?

There are too many questions here of course. And we know that they can be multiplied without end. The real issue, obviously, is the question of the nature of a work of art. One can only suppose that, in seeking to answer this question, a responsible commentator has his mind on questions of the sort just raised. So that we ask of him not more than a series of clues about the answer to those questions, or a number of model solutions following from his account of the nature of a work of art. The

rest is industry, unless unsuspected inadequacies appear in the effort to apply the clues. All the more reason for an economical beginning.

What is a work of art? I do not propose to answer the question in the sense of advancing a statement about the genus and differentia of art. But I wish to make certain preliminary observations about the peculiarities of fine art that would affect substantially any such statement. If I may put it this way, I wish to identify rather than to define a work of art. A suitable beginning may perhaps be made by noting some features about our question. When one asks, "What is a work of art?" the answer is expected to be satisfactory for all of the principal fine arts, to be not trivial, and to be capable of further specification for the sake of the genuine differences among the various arts. Again, when one asks, "What is a work of art?" the answer is expected to be such that anything that meets the qualifications of the definition can be described in such a way that descriptions proffered may be true or false. It may appear obtuse to enter an objection to such a reading, not an objection to the fact that this is commonly intended by the question but an objection rather to the adequacy of the common view. I have attempted elsewhere to show that different and even contrary descriptions of a work of art may, in principle, be confirmed (the process requiring an alternate model to that of truth and falsity)¹ and I shall assume here that this view is a correct one (a view that also claims at least some commonsense acceptance, as in the beliefs that different ages must perceive the same works of art differently and different cultures perceive alien works of art in terms of their own distinctive experience).² It is worth remarking that commonsense usage appears to be self-contradictory here and cannot supply us therefore with reliable tests for an acceptable answer to our question. Finally, when one asks, "What is a work of art?" though it initially appears to refer to the question, "How would we describe something as having the generic properties of a work of art?" the question is ambiguous in common usage, faltering on what C. L. Stevenson has termed "persuasive

¹ "Proposals on the Logic of Esthetic Judgments", for presentation before The Fifth Inter-American Congress of Philosophy and the Second Congress of the Inter-American Society of Philosophy at Gallaudet College, Washington, D.C., 8th-12th July, 1957; "The Problem of Relevance in Esthetic Criticism", for presentation before the annual meeting of the American Philosophical Association (Eastern Division) at Harvard University, Cambridge, Mass., 27th-29th December, 1957.

² Further *prima facie* evidence for the thesis will appear throughout the discussion that follows.

definitions".¹ Since, in principle, evaluative questions must be raised about materials antecedently described without prejudice to evaluation, we shall assume that "What is a work of art?" is to be answered in a merely descriptive sense (with due qualifications imposed by the possibility of defending even contrary descriptions of a work of art).

I have said that I shall not here attempt to defend the thesis that, in principle, even contrary descriptions of a work of art may be confirmed. Let me cite merely a set of remarks from a recent book review by Moses Hadas that will suggest the familiarity of the thesis and will allow us to take a step forward as well:

The central innovation in this stimulating study of Sophocles' tragedy is the suggestion that Oedipus is meant to represent Athens, not merely in a figurative sense as a text for a homily, but as a literal equation . . . Too bare a summary cannot do justice to the cogency of Mr. Knox's proofs or to his valuable incidental insights. His interpretations must be welcomed with thanks, provided we do not (as he would doubtless insist we should not) exclude other modes of interpretation.²

The expression, "other modes of interpretation", I take to indicate Hadas' view that his study of the *Oedipus* cycle and the critical literature surrounding it leads him to conclude that there are a number of plausible interpretations of the cycle among which some (including now Knox's interpretation) are not reducible to, or subsumable under, other interpretations. Clearly, it would serve no purpose to speak of a comprehensive theory that would include, by mere conjunction, constitutive sub-theories not otherwise integrated; the device would be a mechanical solution for every such puzzle and would contribute nothing additional to the understanding of the work of art.

But the odd thing is, granting that such interpretations are what we have in mind when we speak of deliberate aesthetic interest in a work of art, that it is precisely our aesthetic interest that threatens our effort to identify a work as a public object. In so far as we attend to different interpretations, or descriptions, of a work of art regarded as confirmed (the terms may be taken as synonyms in this context), we should be inclined to speak more simply of different works of art. That we do not wish to is clear enough; but if contrary descriptions of an ordinary natural

¹ Cf. *Ethics and Language* (New Haven, 1944), "Interpretation and Evaluation in Aesthetics", in *Philosophical Analysis*, ed., Max Black (Ithaca, 1950), and "On 'What is a Poem?'" , *The Philosophical Review*, lxi, no. 3 (June 1957), 329-362.

² Review of *Oedipus at Thebes* by Bernard M. W. Knox (New Haven, 1957), *New York Times Book Review*, 16th June, 1957.

object, say, an oak tree, were regarded as confirmed, we should then be obliged to insist that the descriptions were true of two different objects. The only shift possible, granting the conditions we have imposed, is to provide a model of confirmation other than that of truth and falsity. Let us at least conclude that, even though it is what makes a work of art distinctively what it is, the aesthetic design of a work of art (that is, what we describe or interpret in the aesthetic mode of attention) cannot in itself enable us to speak of a single work where we want to, as we surely do among the questions initially posed. The argument drives us to suppose that the properties which distinguish works of art as such are different in certain fundamental respects from the properties of natural objects; and in the light of our sense of philosophical difficulties in aesthetic discourse, this is actually a welcome proposal. Certainly, we seem to have accommodated, in part at least, the circumstances in which, for example, we wish to speak of significantly different recordings of Bach's *Brandenburg Concerti*; the fact that these different recordings will show variant aesthetic designs and even different combinations of instruments does not yet force us to deny that it is the *Concerti* that are interpreted in these recordings. So our discussion is all to the good. Commonsense usage in fact actually presents such comments as, "How differently Horowitz plays it from Rubenstein" and even, "You'll have to practice a lot if you want to play it as does Horowitz".

If we turn to the other end of our topic, if we turn from that which makes a work of art what it distinctively is to that out of which it is somehow formed, there is an incredibly large number of minor puzzles to be found ready to engage our energies without real benefit. There is, to be sure, a paradox in the discussion of the physical medium on which a work of art depends to correspond with that already noted in speaking of the esthetic design. We can, in principle, describe, without risking ultimate contrariety, the properties of a physical system that serves as art medium; but such a system is not yet a work of art. It almost seems therefore as if the work of art must elude us. But we really can, by combining what we already know about the design and what we have just observed about the barest physical art medium—say, a lump of granite as a sculpture or a spread of pigments in a painting—actually advance to a first proposal about the identity of a work of art. To denote a work of art must mean to denote a physical object or objects that can be construed as art medium, to which, that is, an aesthetic design can be defensibly imputed (however that may be done). And moving thus far

only, we can say without difficulty that if we impute alternate and even contrary designs to the medium, we are merely doubly confirming the presence of a work of art—we can point to the physical system that serves as common medium for all of these designs.

This is the most important advance. Everything else that one can say is intended to make adjustments and concessions, to clarify implications and additional assumptions, to solve problems of more detailed and particular importance.

“To denote a work of art” is an elliptical expression. The work of art must be the physical system functioning as art medium, in such a way that a design may be imputed to it. We must speak in gross terms at this point for the sake of simplicity. If we say that we perceive the physical medium through our senses, we may say that we perceive the corresponding aesthetic design through our imagination. To see the force of the distinction (however incomplete our statement), let the admittedly more complex case of literary art be considered for a moment; if we see Raskolnikov moving through his crime (and we do see him as we read Dostoevski's text), we must surely see him through our imagination. Now, in order to behold something as a work of art, we must combine in the same perception the sensory seeing of the physical medium and the imaginative seeing of an aesthetic design; the sensory properties must be imaginatively seen; and so we may speak of a third mode of perception, imaginative perception. If we are to say that the work of art exists, it exists only in the domain of imaginative perception; and it may be denoted only there. It is for this reason that we cannot really point to it and must move in a roundabout way; the fact also serves to render more congenial the distinction between natural and artistic properties, on which we founded the curious capacity of works of art to assume multiple and even contrary designs. That the relationship is relatively stable between medium and design is hardly in doubt. We have only, to be convinced, to recall the great labours of artists like Van Gogh, daubing physical pigments on a physical canvas, or else the fortunes that have changed hands in the sale of these same canvasses. And it is for reasons such as these that we all choose to speak flatly of the existence of a work of art. But we must understand the possible ambiguity of speaking of physical objects and art objects in the same breath. We cannot denote them in the same way; the principal clue is that the second always depends on the first, but the contrary relation does not hold.

This last observation allows us an economy. The ambiguities that arise in speaking about works of art will not fail to include

all the general ambiguities that arise about ordinary objects in nature or about the use of terms in our language. We can afford to confine our attention therefore to the special difficulties that arise in speaking of works of art. I mention the point because C. L. Stevenson has, in a recent essay, usefully applied Peirce's distinction between tokens and types to certain ambiguities arising in speaking about a poem,¹ but ambiguities in speaking about a poem in a way that is indifferent to its status as an object of esthetic interest. In this regard, he examines such remarks as, "There are many poems that are about classical mythology" and "Each student was expected to write down the same poem that the teacher recited".² These surely raise legitimate issues but we could easily substitute statements in which the same ambiguities would remain and all reference to art would disappear.³ The questions Stevenson raises actually occur in subtler forms he himself touches on in discussing the matter of translating a poem.⁴ I should like to avail myself of Stevenson's version of the token-type ambiguity, because of its advantages, but with certain important reservations. Let us adopt the following phrasing (disregarding frankly the full context in which the statements occur):

Now when we want to speak of [token poems] we obviously have a number of other terms available; we have such terms as "manuscript of a poem", "copy of a poem", "recitation of a poem", and so on. It may seem, then, that we should reserve the term "poem" itself for the corresponding type... it would... preserve a clear sense in which the copies, and so on, are of the poem. They are of it in the sense in which various individuals are of a certain kind, the relation being that of membership in a class.⁵ Peirce it must be remembered regarded tokens and types as signs: we shall want to remain neutral on the matter of the adequacy of a semiotic theory of art.⁶

¹ "On 'What is a Poem?'" , pp. 330-333 I find that Richard Rudner has made reference to this usage in an interesting exploratory essay on C. I. Lewis's handling of related difficulties; cf. "The Ontological Status of the Esthetic Object", *Philosophy and Phenomenological Research*, x, no. 3 (March 1950), 380-388, esp. 385. Compare also Stevenson, in Black, *op. cit.* pp. 347, 353 and Rudner, p. 387. ² *Op. cit.* pp. 331-332.

³ Stevenson implicitly acknowledges this; cf. *ibid.* p. 330.

⁴ *Ibid.* pp. 336-338.

⁵ *Ibid.* p. 331. Stevenson maintains the token-type distinction but wishes to preserve the ambiguity (the richness) of ordinary usage.

⁶ Cf. *Collected Papers of Charles Sanders Peirce*, ed., Charles Hartshorne and Paul Weiss (Cambridge, 1933), vol. iv, par. 537, cited by Stevenson. Stevenson's discussion may be taken to be ambiguous on this matter—for example, his closing remarks.

When he comes to solve the question of translation, Stevenson interposes a third notion, "megatype", in the following way :

Two tokens will belong to the same megatype if and only if they have approximately the same meaning ; so it is not necessary that the tokens belong to the same language or that they have that similarity in shape or sound that makes them belong to the same type. Thus any token of "table" and any token of "mensa", though not of the same type, will nevertheless be of the same megatype. The distinction need not be restricted to individual words, of course, but can be extended to larger linguistic units, including poems.¹

Now, to remain neutral on the semiotic theory of art and to accommodate our own previous observations of the peculiarities of fine art and to anticipate somewhat further difficulties, we shall have to make the following adjustments : (a) read, with appropriate grammatical alterations, "impute a design to" for "have the meaning of";² and (b) take it that two tokens belong to the same megatype if and only if they share approximately some design from the range of alternate and even contrary designs that may be defensibly imputed to each ; or the designs of both, however different, can be defensibly imputed to the megatype signified by an art notation. Furthermore, to paraphrase Peirce (and misusing his terms still as semiotically neutral terms), "in order that a type or megatype may be used, it has to be embodied in a token ; such a token is an *instance* of the type or megatype".³

The apparatus is now very convenient. We normally wish to refer to a poem through the text of some critical manuscript ; if we need to invent a term, we may call it not merely an instance of the poem but the prime instance. And this surely corresponds with the practice of historical and critical studies in poetry. Other printings, including variant versions and translations, may then serve as additional instances of the megatype. We may even speak of good and bad translations in a non-evaluative sense, if we agree on the prime instance of a poem ; this simply calls for specifying a procedure for applying (b), noted above. We may not agree on a prime instance, as in collecting variations

¹ *Op. cit.* p. 337.

² "Design" is offered here as a neutral term, in the sense of composition, that which we describe as esthetically significant ; it may include the enumeration of meanings as Stevenson proposes and also the organization of non-linguistic elements.

³ *Loc. cit.*

of folk songs, but the applications of (b) will allow us to decide to what extent two token songs are instances of the same megatype.

The notion of the prime instance is again of signal importance in the plastic arts, but in a novel way. Reproductions correspond either to copy tokens (as in plural sculpture castings or etchings from the same plate) or to translation tokens (as in reduced-scale colour lithographs of great paintings). In the case of sculpture casting or etching, an adjustment may be made. We need not actually insist on a single prime instance; there is a causal factor that may be appealed to by which to designate a set of prime instances. But we see here that an important difference develops nevertheless. In the literary arts, the prime instance is used only as a device for controlling the enumeration of tokens for a given megatype. We are inclined to identify *the* poem with the megatype poem and *use* it in the form of an acceptable token. Even when we have before us, say, the first quarto and first folio editions of Shakespeare's *King Lear* do we speak this way; that is, even when we have before us prime instances. Let me quote, to illustrate this usage briefly, from Professor George Duthie's remarks on the *Lear* manuscripts:

There are two substantive editions [*i. e.* editions 'which are not derived as to essential character from any other extant edition'] of *King Lear*—the first quarto edition, published in 1608, and the first folio edition, published in 1623. In Chapter II of the introduction to my edition of the play I have argued that we must accept the view that *F Lear* was printed from a copy of *Q* which had been brought by a scribe into general agreement with an authentic playhouse manuscript, doubtless a prompt-book. And in Chapter II, section (i), I have argued that we must accept the view that at some stage the *Q* text was memorially transmitted, *i. e.* that it is a reported text. In any given case, then, in which *Q* and *F* have different readings, we must assume that the *F* reading is genuine and the *Q* reading corrupt, *unless there is in that particular case, a definite reason for supposing that this is not so.*¹

But in the case of a sculpture or a painting, we identify *the* work of art with the megatype sculpture or painting as it is actually embodied in the prime instance; and even when we speak of original copies of an etching, which returns us apparently to earlier usage, we mean to preserve the usage just specified. This is interesting because it clearly indicates that we do not

¹ *Elizabethan Shorthand and the First Quarto of 'King Lear'* (Oxford, 1949), p. 1; italics mine.

identify a work of art in precisely the same way in the different arts.

Nor is it sufficient to explain the difference in terms of such considerations as market value, since a first quarto *Lear* may prove a valuable piece of property as readily as an original painting. The clue to the difference seems to lie with the importance of the actual physical marks of any token for its megatype. The difference between a printed poem and a painting, in this regard, is quite important. It is possible to view the printed poem as a *notation* for the poem; it is not possible to view an original painting as such a notation.¹ It is important to see as well that this does not at all mean that a physical medium is not required by a poem; surely, a poem depends on what we may call sound or voice just as a painting depends on pigments. But the pigments can only be seen as constitutive of the painting, whereas the written words of a page of poetry may be ambivalently taken to be constitutive of a poem (as in an actual token poem) or as merely a notation of what would be constitutive of any instance. The very same difference is sustained through reproductions or translations in either art. In literary art, we *refer* to a prime instance in order to determine other instances of, say, a megatype poem; and any of these will then be on a par with the prime instance. But in painting and sculpture, we *mean* by a painting, say, *only* the megatype painting as it is embodied *in* the prime instance; nothing else will do. I think our ordinary language bears this out. A student may be assigned the task of reading a poem and he will do so by consulting at random any of a very large number of alternate printings and sometimes, as in Shakespeare, even variant editions; it is not really usual to say that he consults a likeness of the poem, or some such expression. But we have any number of locutions designed to caution against identifying, say, a cheap reproduction of a painting with the painting itself—e.g. "Let me show you a slide of Van Gogh's *Sunflowers*; it's not quite like the original, there's too much lemon and not enough orange in the petals" or "You can get an idea of the painting from this postcard picture". The point is, and it is certainly an important point in the appreciation of art, that the skill which we prize in the poet is his arrangement of words that will sustain interesting aesthetic designs, not some peculiarly accidental physical notation of the poem; but the skill of the painter or sculptor is precisely what would be (but no longer can be considered as) the counterpart of the poet's

¹ We must make an adjustment here, as we shall see.

or editor's notation, it is his execution *in pigment and stone* that counts. The distinction may be summarized in this way: we can use a poem by locating instances of the poem but we can use a painting only by locating prime instances of the painting.¹ The distinction allows us to accommodate translations and lithograph likenesses as well. A translation and a lithograph likeness may be tokens of megatype art objects but not of type art objects. In a painting (I am addressing myself here to the proposals Stevenson has made regarding poems),² we require that tokens be of the same type as well as megatype to be said to be instances of the same painting; usually, there will be a unique object. Even apart from translations, however, this is not *required* of poetry. And because of this usage, even when translations fail to provide for portions of the design of the megatype poem, we continue to speak of English translations, say, of the *Odyssey* as instances of the poem just as we would of versions in the original Greek or in French and German translations.

It is easy to be misled here. The fact that language, generally speaking, is translatable and gross habits of aesthetic interest in literary art attend primarily to the translatable aspects of poetic language incline us to speak about translations as developed above. But if we share, with the New Criticism, an interest in the "texture" of a poem, "the quality of the poem beyond the merely paraphrasable rational content; the heterogeneous detail of the situation, including metaphor and meter",³ we must revise our account of the location of an instance of a poem by revising our view of the kinds of detail that will be assigned in the design of a poem. A difference still remains between painting and poetry, though now the execution of the poet is conceived not only in terms of words but also in terms of physical sounds and rhythms; and this seems to agree somewhat more with ordinary language usage. The difference we now see, which has suggestive implications for other arts, is this: the poet, because he chiefly composes with words, may be said to *select*

¹ I speak here of *identifying* a work of art, *locating* and *using* instances of a work of art, and *denoting* and *pointing* to the physical system on which any instance depends. They are all of course distinctions in denotation.

² *Op. cit.* pp. 337-338.

³ Cf. entry "texture" in *A Guide to the New Criticism* by William Elton (Chicago, 1953, revised); we need not commit ourselves to the present version of the Criticism's view of texture, only to the recognition of a certain kind of quality not reducible by paraphrase. The official Criticism jargon for the error on which the previous view of translation rests is termed the Heresy of Paraphrase (cf. entry "paraphrase, heresy of" in *Guide*); it is interesting that the glossary includes no specific entry under "translation".

his materials from an antecedently well-defined and well-ordered fund (of language)—his creativity chiefly relates to the *arrangements* of these materials; on the other hand, a painter cannot point to such a well-ordered fund—his creativity relates to his original *preparation* of the very materials he finally arranges. This accounts for our indifference, even with the revision mentioned above, respecting different instances of a poem. (The emphasis on texture, by the way, supplies a *prima facie* reason for neutrality on the semiotic theory of art, even with respect to poetry.)

I suggested the need for further adjustment in the case of the plastic arts. There is of course a sense in which paintings, drawings, etchings, and even sculptures may be viewed as notations. A Cézanne watercolour frequently serves as a preparatory sketch for an oil; a Munch woodcut, for one of his oils; a drawing by Henry Moore, a sculpture; a few pen strokes with the names of colours appended, a Van Gogh. All of these however are fundamentally in a different relation from, say, that of a group of related paintings by El Greco (the various versions for example of *Christ Driving the Moneychangers from the Temple*). El Greco's composition may improve from painting to painting, but each is nevertheless viewed as a complete painting and not as a notation. When a piece of plastic art is viewed as a notation, it must thereby cease to be viewed as a work of art; hence our question does not arise. Also, because of the importance of direct execution in plastic art, such a notation must inevitably be viewed as incomplete by any observer other than the artist himself. And finally, the plastic piece construed as a notation cannot, when it is alternately viewed as a work of art, belong to the same megatype as that of which it serves as a schema. A sketch by Frank Lloyd Wright of a building to be erected may of course attract our attention as a work of art; in that case, we no longer view it as a mere notation for some *other* work of art. The contrast with poetry is clear.

Architecture fits in very nicely here. In construction in which direct invention in materials holds our attention, as in medieval cathedrals, we are inclined to treat the buildings much as we do painting and sculpture, emphasizing prime instances. But in much of modern architecture, where mechanical execution has largely superseded the direct invention of the cathedrals, we are prepared to view different buildings as instances of the same megatype; in this latter case, architectural notations tend to be complete notations and, correspondingly, the execution of any token building points to the artist's *arrangement* of antecedently

well-ordered materials. The resemblance between such architecture and poetry is striking.¹

An odd consequence follows for music. In the history of aesthetics, music has been frequently taken to be the fine art *par excellence*, because of the complete integrity of form and content and because of its untranslatability. And in this regard, we should be inclined to suppose that we identify a piece of music more as we do a painting than as we do a poem. But the barest reflection shows that the contrary practice prevails (at least in part). Normally, in concert music we cannot speak of a prime instance; all we have is a music score, and a music score is unambivalently a notation (in contrast to a poem). We see from this that our identification of a piece of music presupposes, as does literary art, an antecedent well-defined and well-ordered fund of materials. And because a music score is a notation, a sign of a work of art and not a work of art itself, and because our admiration for the composer refers to his *arrangement* of antecedently defined and ordered notes, any token performance of the megatype composition noted in the score will serve as an acceptable instance of the music. Just as in poetry, here we use the musical composition embodied in any random instance (say, a recording or a live performance). So, in spite of the fact that the organization of physical materials is more completely the concern of music than it is of poetry (think for example of the persuasive arguments of the proponents of the purist school, however incomplete),² we are inclined to identify a musical composition in the same way as a poem; and this in contrast to painting and sculpture. We may say that, in music, we typically lack a prime instance and have instead a prime notation for possible instances.

Music is, however, somewhat more complicated than we have allowed. The reason has already been given; typically, we have only a music score to point to, we do not have an actual musical composition. And even if we had a prime instance performance and no score, we would ordinarily be inclined to construct a prime score from it and prefer it even to the performance of the composer himself. It is an interesting point to observe that precisely the same attitude is to be found in the dance (where

¹ The comments of Abbot Suger on the alteration and enlargement of the Church of St.-Denis, under his administration, will serve to focus these different conceptions of architecture; cf. *Literary Sources of Art History*, selected and edited by Elizabeth Gilmore Holt (Princeton, 1947), pp. 20-44.

² Cf. John Hospers, *Meaning and Truth in the Arts* (Chapel Hill, 1946), pp. 78-98.

a notational system is now slowly emerging) and in dramatic art. Even when intricate ballets were not scored but merely passed on by authoritative members of a company, even when it was realized that different dancers could not help but interpret a given dance in terms of the idiosyncrasies of their bodies and temperaments, the idea persisted that all such performances were, however distinctive, performances of the same dance. And even though, to be sure, the fundamental movements of some old ballets are hopelessly lost, so that recent productions that have preserved the same name and some gross features of a remembered ballet are really new dances, the intention at least is clear. Now all of these are more distinctly performing arts than poetry and fiction and painting and sculpture ; or if poetry and fiction be viewed as performing arts (as well they might), the relative fixity of even their " texture " and our relative indifference to deviations from some generalized recitation would confirm our use of random tokens as instances. Given a prime instance of a poem therefore, we can judge whether any other token poem, variant or translation, is an instance of the same megatype poem. But given a token musical performance or dance or a dramatic performance, we face some further difficulties. We seem to be disinclined even to speak of a prime instance in the case of these arts and so we may suspect some important difference between them and literary art.

The fact is that when we are able, in any art, to start with a prime instance we are in a position to decide whether any other token belongs to the same megatype ; we have only to determine all the designs that may be imputed to the prime instance. And should a translation of a poem, say, permit of a distinctive design, we have only to inquire whether and to what extent that design may be imputed as well to the prime instance. But when we start with a prime notation only, and even disallow prime instances, we cannot control the identity of the work of art in the same way.

The important clue to our shift of emphasis here is the contribution of the performing artist ; it is clearly much more significant in the arts of music, dance, and drama than in the others already considered. We recognize that the performer *adds* in a distinctive way to the compositions sketched in the score or notation ; at the same time, we wish to preserve the sense in which, say, two performances of *Macbeth* are instances of the same megatype. That is, just because the performer *adds* to, rather than *invents de novo* an artistic creation, we wish to preserve the sense in which significantly different performances

may be instances of the same megatype. Our effort to identify a work of art however need not correspond with our primary aesthetic interest—we may, for example, be interested in the originality of the performer alone. Hence in the performing arts, the ease with which we may locate a token performance as an instance of a given megatype depends on the ease with which we can identify that to which it adds its own distinctive contribution.

Drama is of course in a privileged position among these arts because it enjoys all the precision of identity which accrues to the literary composition on which it depends. Thus, regardless of the variation among dramatic performances, if a performance can be regarded as an instance of the megatype literary composition, all of its dramatic innovations will be discounted; that is, they will be discounted in the sense that widely variant dramatic innovations will not disqualify performances from being tokens of the same drama—they will *not* be discounted in the sense that it is precisely in virtue of these innovations beyond the literary composition that inclines us to speak of a drama. If we did not make this distinction, it would be well-nigh impossible to contrast fiction and drama, since in reading fiction we recreate imaginatively the characters and action of the piece. Hence the importance of conceiving a written text as a score when we have drama in mind; a written novel (as in an argument already advanced) may of course be taken to be a token novel. It is the staging and acting that attracts us primarily in a dramatic performance; but Orson Welles's modern dress *Julius Caesar* is not less an instance of the megatype drama than a performance of the Old Vic, regardless of which we may prefer, nor of the Old Vic's less than of the Globe Theatre performance (which must surely have been quite different). Since, however, in drama we usually possess a prime instance of the literary composition, we can be more detailed on the question of the extent to which two token plays are instances of the same megatype. In addition to this consideration, we can always decide the extent to which two performances share peculiarly dramatic features; we may determine, that is, not merely that two performances are instances of the same megatype drama but that they are instances, say, of the Old Vic megatype performance of the megatype drama.

Now, it is important to notice that when a literary composition is regarded as a notation for a drama, we are inclined to take an extremely relaxed view of stage instructions and the like. This reinforces the observation we made that it is sufficient to view a token performance as an instance of the literary megatype. The point is of course to give the greatest latitude to the ingenuity of

those who are to mount an actual performance—which is our chief interest in drama. We have no difficulty therefore in viewing Lawrence Olivier's film versions of Shakespeare's plays as instances of those plays. From the same view, we find it acceptable also to treat literary studies, say Bradley's *Shakespearean Tragedy*, as dramatically relevant.

But in music and in the dance, we lack the prime instance that drama possesses in virtue of being dependent on literary art (art forms like pantomime will of course resemble dance in the details under consideration). The result is that we are left merely with a prime notation at best. If we have no prime notation (as is usual in folk music and folk dance), we can do no more than determine the extent to which two performances are instances of the same megatype; there is that difference, for example, between identifying a prelude specifically by Rachmaninoff and identifying merely the folk song *Barb'ry Allen*. The same limitation that applies to folk art very frequently applies even to more formalized balletic and modern dance performances.¹

We may perhaps fix the distinction of music and dance if we consider the following contrast between a dramatic performance and a dance performance. Usually, the dance employs music as drama employs literary texts. But, though we can identify a drama by reference to its literary text, we cannot do the same for the dance. For one thing, it is always possible to compose two different dances for the same musical composition, as has been done, for example, with some of Purcell's music; we would not, however, speak in this manner even of such different performances as Stanislavski seems to have provided of Chekhov's plays and of such as would agree with Chekhov's view of his own plays. For another, the medium of literary drama *includes* the medium of its corresponding literature but the medium of music does not include the medium of the dance nor that of the dance, of music. The dance and music share types of physical properties in common, rhythm for instance, in virtue of which they may *accompany* each other. We are even prepared to speak of an unaccompanied dance as an instance of the same megatype as a musically accompanied dance; nothing comparable appears in drama, unless it is a pantomime version of a drama, and then precisely we must identify it in the same way we identify a dance and not in the way we identify a poem.

In the concert dance and in folk music, as we have already suggested, it is not uncommon to lack both a prime instance and a

¹ A further adjustment will be made below.

prime notation. It is even conceivable that two anonymous token poems of the classical Greek period be discovered, which, though they appear in different dialects, be judged to be instances of the same megatype—that is, we may lack here also a prime instance and a prime notation. But we should have little difficulty, in accord with our discussion earlier, in deciding that they are instances of the same megatype. In the drama, as we have seen, two performances that do not share a design in common may nevertheless be said to be instances of the same megatype if their respective designs may be imputed to the literary composition on which they depend. The element of performance, it must be remembered, however, adds to the composition. In the dance, invention in body movements tends to make each performance distinctive; even in drama, the distinction of each performance is regularly admitted. Were the dance like painting and sculpture, we should merely say that each dance is a unique work of art. But dancing is a performing art in a sense that distinguishes it from these other arts. And so, even though the direct execution of each performer is what interests us in the dance, we are always inclined to think of dance performances as, at least potentially, performances based on a score or notation. A dance notation is apparently now forthcoming, so that we may expect, increasingly, that a prime notation will be available; in that case, the identification of a concert dance will resemble very much that of a musical composition. Short of this, it is still possible (and we wish) to speak of different token performances as instances of the same megatype dance. And yet, if each performance is quite distinctive, we may seem to be stalemated here, since the design of each will not be able to be imputed to the other. The clue, however, has already been provided. If we wish to assert that two performances are instances of the same megatype dance, we must be prepared to formulate a dance notation for which either performance will be a plausible token. The relation then will be much like that which holds among alternate dramatic performances.

To see how distinctive the performing arts are in this respect, we have only to consider that it is altogether possible to formulate a notation for which the three versions of El Greco's *Christ Driving the Moneylenders from the Temple* would be plausible tokens; it would seem possible to attempt something of the same sort, say, for various paintings of the De Stijl school or even for Picasso's *Ma Jolie* and Bracque's *Man with a Guitar*. But we do not wish to do that in the plastic arts and we do wish to do it in the dance. The fact is that we have an option sometimes and

that there are, ordinarily, reasons of weight for preferring to treat performances as instances of a common megatype rather than each as a prime instance of its own distinctive megatype. In drama, we have seen that a common text suffices; in folk art, the very homogeneity and solidarity of the culture suffices; in the concert dance, it may be the presence of a repertory company or some such consideration. Certainly, the complexity of the tokens that resemble each other will be a contributing factor; also, the distinctive way in which we identify works of this or that general kind of art.

To recapitulate briefly: we do not identify works of art always in the same way. A considerable range of variant criteria can be found in ordinary usage. Sometimes (as, typically, in the plastic arts) we identify a work by identifying a megatype as it is actually embodied in a prime instance. It is interesting to speculate that, if we could actually reproduce a fully accurate copy of a painting by some mechanical process, we would probably drop the strict requirement; that is, we would identify paintings somewhat more as we do poems.) Sometimes (as, typically, in the literary arts) we identify a work by identifying the megatype of a prime instance and use that megatype in any suitable instance. Sometimes (as, typically, in music) we identify a work by identifying the megatype signified by a prime notation and use that megatype in any suitable instance. Sometimes (as, typically, in drama) we identify a work by identifying the prime notation, on which it depends, and hence also the megatype signified by that notation and use the megatype in any suitable instance that exhibits as well the general properties of the kind of art the work in question is said to belong to (*e.g.* drama, dance, music, poetry). Sometimes (as, typically, in the dance and in folk art) we identify a work by constructing a notation, the megatype it signifies being such as will permit us to use given tokens as instances of that megatype. A certain vagueness attaches therefore to the last case.

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V.—PARTLY SO AND PARTLY NOT SO

BY RAPHAEL DEMOS

IN the course of daily talk, phrases like the following crop up : Smith is partly clever and partly stupid ; Jones is partly good and partly not ; Brown is a mixture of opposites—both gentle and fierce. So Plato was concerned about occasions when we are both repelled and attracted by the same object, as when we are drawn to the sight of a corpse, yet at the same time are angry with ourselves for doing so (*Republic*, 439e, ff.). Also Aristotle describes choices arrived at under threat, as partly voluntary and partly involuntary (although more voluntary than involuntary, *Nic. Ethics*, 1110a 12-19). Now, about all such statements there is this striking fact : they seem to violate the law of contradiction, in that they assert the conjunction either of contradictories or of contraries. The statement that Jones is partly good and partly not entails (a) that Jones is both good and not good, and (b) that he is neither. Or perhaps the point should be made in a milder fashion. The statement is not quite a contradiction as it is in " Jones is and is not wholly good " ; but it neither is quite non-contradictory, as with " Jones is wholly good and not non-good ". In other words, our statements seem to be partly contradictory and partly not so.

Clearly there are escape-clauses here. Smith's being partly clever and partly stupid may be explained to mean that he is clever at fixing the radio but stupid at grasping the theory behind it. Jones is good to the members of his family but mean in his relations to everybody else. Plato, too, accounted for the simultaneous repulsion and attraction, by dividing the soul into parts, one of which repels and the other attracts (*Republic*, 439e, ff.). Nevertheless, I doubt that his solution is adequate ; for, on his view, it is the whole soul which acts whether as reasoning or as desiring. But we can find a sure escape in such statements as " the meadow is partly green and partly not ", because probably what is meant is that part of the meadow is wholly green and the rest of it is brown. In all such escapes the conflict with the law of contradiction is eliminated by the specification of ' different respects '.

Nevertheless this gambit does not always win the game. When I say that Morrison was only partially successful in his enterprise, the statement need not mean that I divide the enterprise into two parts, saying that he was successful in one part and a failure in the other. And when I say that this sheet of paper is partially white, I need not mean at all that part of the paper is

wholly white and the other part wholly not white ; I may mean that the whole of the sheet of paper is partially white, such that the sheet of paper overlaps the two classes (white and not white) without being wholly contained in either. Here the formula 'mixture of opposites' seems to apply. So with Smith's cleverness ; we may mean that in any one particular situation Smith is partly clever and partly not. Now, it is possible to construe Jones' partial goodness to mean that Jones is generally good, *except in certain circumstances*. But these circumstances would be indefinite ; there could be no exhaustive formulation of them. We would be (or might be) obliged to say that Jones is good except in circumstances, a,b,c, and so on. Or, take Aristotle's account of the voluntariness of a decision when a threat is hanging over a man ; in saying that a decision under such circumstances is partly voluntary and partly not, Aristotle means that I decide from fear and also from a rational choice of alternatives. In short (as with Plato), there are two distinct parts of the soul which are operating, reason and passion (*pathos*). But I cannot agree ; when I am being influenced by fear while simultaneously engaged in reasoning (deliberating), can it be said that I am genuinely reasoning ? There is a mixture here resulting in a new product. Well, why not ? There is a distinct new 'part' of the soul whose operation we may call 'freasoning' (fear + reasoning ; after all, we do speak of intermediate or mixed colours). And so, there would be no contradiction ; when a man 'freasons', he does not do so partly ; he just 'freasons'. This looks plausible, but I will try to show in a later paragraph that it does not work.

But even to grant my point that the adverbial 'partly' is not divisible into parts does not mean that the law of contradiction is violated in all statements of ordinary language. The very statement itself, "X is partly so and partly not so" obeys the law of contradiction, for it excludes the statement "It is false that X is partly so and partly not so". True enough, but the difficulty still remains. For the statement ("X is partly so and partly not so") permits one to say that it is true 'neither that X is so, nor that X is not so', and also to maintain that both so and not so are attributes of X.

Another group of phrases in ordinary language which seem to make trouble for the law of contradiction are what I will call 'degree-phrases'. For instance, we say, "The water in the kettle is not quite hot" ; "This man is very good" ; "Mr. B almost succeeded". Now, when the water is not quite hot, is it hot or is it not hot ? Traditional logic requires that a subject either has or has not a given attribute ; but ordinary language

interposes, between subject and attribute, modifiers like 'very', 'much', 'little' (as an adverb), 'more', 'most' which seem to disrupt the union (or divorce) between the two and render it indeterminate. Logic lays down the disjunction, heaven or hell; ordinary language introduces a purgatory. Unlike the first group, this new group uses only *one* attribute. Instead of 'partly this and partly that', we have only 'this' with the proviso that 'this' (the attribute) is possessed by the subject *approximately*, or to a degree. Such attributes function now as norms rather than as merely descriptions; as limits or as Platonic ideals. In fact, ordinary language seems to echo Plato's doctrine that empirical things copy their forms imperfectly or partake of them. The famous case is his example of the two sticks which may not be said to be perfectly equal (*Phaedo*, 74f, ff.). So in ordinary talk we say "This man is good but not quite good". Thus, an incidental conclusion of this paper is that Plato's doctrine of the mixture of being with not being (as applied to material objects) reflects ordinary language and probably was suggested by it. However, according to Plato, an empirical object can *never* imitate a thing completely; yet, in ordinary language it makes good sense to say "X was completely successful", "This water is quite hot", etc. (although commonsense also agrees with the saying that even Homer sometimes nods). Perhaps, then, Plato started with the comparatives of ordinary language ('more', 'most') and with other such terms as 'very', 'less', 'much', 'not quite', and then proceeded to extrapolate beyond the uses of ordinary language.

To digress somewhat, Plato was determined to avoid any conflict with the law of contradiction. His aim was to achieve precision even in the description of the empirical world. We have noted already how he dealt with the problem of simultaneous opposing desires in the same person. In addition, he was exercised over such other apparent contradictions as are involved in statements like "Simmias is both tall and short". He solved this problem by introducing the notion of relative terms (*πρός τι*); thus Simmias is tall in comparison with B, and short as compared to C (*Phaedo*, 102b-d; also *Republic*, 438b-c). Nevertheless he found himself obliged to admit contradiction in the area of attribution-statements, as when we assign a predicate to an empirical subject. It may be that Plato's doctrine to the effect that the empirical world lies between being and not-being does not make sense in the case of existence-statements; but it does make sense with statements about qualities. Plato's other doctrine of self-predication has been criticized as absurd; as when he says that equality is equal and justice is just. But the doctrine has at

least a negative merit, namely, that *if* one could legitimately predicate a form of itself ('justice is just'), the copula would not be subject to any modifier. The self-predication of the forms is a foil to the participation of *things* in forms; the first is unqualified, the second always qualified ('not quite', 'approximately').

Here, two remarks are in order. (a) Value-words (good, successful) make more trouble than ordinary descriptive words. (b) And among the latter, it seems that it is quality-words, *not* substance-words, which raise logical eyebrows; or adjectives *vs.* nouns. As Aristotle says (*Cat.* 3b 37), no particular substance is more of a substance than another; no one ox is more of an ox than another ox. But it is different with heat or humidity or pitch, where we talk of varying degrees of intensity. Plato would seem to agree on both points. Thus he says that words like goodness and justice (value-words) are unclear, whereas "when one says 'iron' or 'silver', we all understand the same thing" (*Phaedrus*, 263a). And, speaking of substance-words, Plato says that a word like 'finger' raises no perplexities (*Republic*, 524d, ff.). But I don't think that ordinary language always makes a comparable distinction between thing-words and quality-words; we say, "B is not a real man", or "B is not quite a man".

I have distinguished phrases of the sort 'partly so and partly not so' from what I have called 'degree-phrases'. But it may be objected that the two are mutually translatable. Thus, "X is partly clever and partly stupid" may be translated into, "X is fairly clever" or "X is not very clever". Yet there are cases when the translation will not work. When I say that now I like ice cream less than I did some years ago, I do not mean that now I partly like and partly dislike ice cream; it does not follow at all that because I like ice cream less, I dislike it. I just like it less. And what would be the translation of "Smith is more successful than Jones"? Would it be: "Smith is partly successful and partly not, Jones is partly successful and partly not, but Smith is more so"? This is absurd. Or shall we say, "Smith has success₁ and Jones success₂"? This would not be right either; there is only one attribute (success) and not two (contradictories or contraries).

Another temptation is to equate degree with part; that is, to interpret a statement like "A was not quite successful" to mean that A enjoyed a part of success, and enjoyed it fully, without a 'more' or 'less'. But attributes can have no parts. As Plato argued (*Parmenides*, 131b-c), one cannot divide an attribute in the way in which one may be said to divide a blanket when it is spread over different sleeping persons, such that one part of the blanket

covers one person and the other parts other persons. Indeed, the notion of degree is quite different from the notion of part. The fact that the nose is a part of the face does not entitle me to say that the nose is a face (*Protagoras*, 329d-e), while, on the other hand, a man who has enjoyed a degree of success may be said to have been (approximately) successful. Furthermore, the notion of part as applied to a quality does sometimes indicate the relation of species to genus; thus white as a part (species) of the genus colour; but a degree of something is not a species of something. More heat is not another kind of heat.

A different move in order to bring degree-phrases into line with the law of contradiction might be this: When I say that the water in the kettle is not quite hot, I may explain my statement by saying that the water is just warm or just lukewarm. Quite so, but what about the stages in between hot, warm, and lukewarm? Well, the answer may be that, as the water in the kettle is getting hotter and hotter, at any moment the water has a particular quality and has it without any adverbial qualification. This answer leads us straight into Zeno's den, with its paradoxes. But Zeno apart, the nuances of quality would be uncountable (not to say infinite) and no amount of words in ordinary language could take care of the situation. We might be able to say that, from the point of view of God, the world is determinate; that, further, an ideal language would be able to express the world in definite unqualified copula-words. Nevertheless, it would still be true that ordinary language would be unable to avoid phrases such as 'not quite warm' or 'almost lukewarm'.

Thus, I don't believe that indeterminateness can be eliminated from ordinary language by any device operating from within it. And when I speak of indeterminacy, I am not referring to vagueness or to open-texture-words. I have in mind the two groups I have mentioned: 'degree'-phrases, and phrases of the sort 'partly so and partly not so'. Of course scientific language abolishes the messiness of ordinary language, replacing it with neatness and precision. While ordinary language is somewhat delinquent (although certainly not anarchical), scientific language is law-abiding—the law in question being the law of contradiction. Science does the trick by the use of number words, but it pays a price—it has to abandon quality-words learnt from observation. "Less than three" *must* mean two or one or zero (in the series of integers) and such phrases are exact in the sense that they must be true or false when asserted of a subject. Numbers have queer properties not found either in qualities or in degrees. (a) The numbers two and three are alike in being numbers; but their

difference too is a difference *in number*. The same concept does the job both of the genus and the differentia; in other words, the distinction of genus and species does not apply to numbers (and figures; Aristotle, *De An.* 414b 20-32; *Met.* 999a 6-13). (b) The number two is smaller than three and is also a part of three; thus 'degree' and 'part' are mutually compatible.

So now ordinary phrases like 'not quite hot' or 'partly white and partly not white' are replaced by such exact phrases as 'ninety degrees of heat' or 'so many vibrations of light'. Certainly, at this point, 'heat' no longer stands for a *norm* but for a *scale* with varying numerable intensities. Or rather the word 'heat' is replaced by the word 'temperature'. In this way, we get rid of modifiers like 'very', 'more', 'much', etc., obtaining statements instead in which the predicate is simply asserted or denied of the subject. Although it might appear that a word like 'temperature' is a quality-word, the appearance is deceptive. 'Temperature' is not a word reporting anything we observe, although it yields inferences about observations.

Nevertheless, even science can not go all the way in its law-abidingness. (a) I doubt that the operation of numbering can be applied to value-words; for instance, in answering the question "how good is he"? The same is true of psychological words like pleasantness. (b) A more fundamental limitation to the powers of science derives from its dependence on report-sentences. There must be a reading of instruments: barometers, thermometers, clocks, scales, and so forth. And in these readings there is always a margin or error; for instance, a report of a particular reading would take the form 'two degrees plus or minus'. So what is the degree of temperature? The answer is: 98.5 more or less. Science, which begins with the indeterminacy of ordinary language (with the purpose of eliminating it) succumbs to indeterminacy at the end. Doubtless, the scientific theories and formulae are exactly phrased; but the job of verifying them brings back imprecision. (I am leaving out the uncertainty principle in physics and the implications of Gödel's theorem for mathematics; these are topics which I am not competent to discuss.) Now, mathematics does not include any report-sentences; and I conclude that the law of contradiction applies (without exception) to *no* language except that of mathematics; in short, it applies only to what Plato called the realm of transcendent forms. At least in the kingdom of ordinary language, the law of contradiction is far from being an absolute dictator; at best, its authority is partly that of a reigning and partly that of a ruling monarch.

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VI.—THE COGNITIVITY OF MORAL JUDGMENTS

BY PETER GLASSEN

IN this paper I want to discuss a question that has exercised moral theorists considerably during the last few decades, the question, namely, of whether moral judgments are cognitive or not. Throughout the history of ethics the view that has generally prevailed—indeed, been taken for granted—is that they *are* cognitive; but with the advent of the logical positivists and their sympathizers this view has lost a good many of its supporters, and even those who still hold it sometimes seem to do so with doubts and misgivings. Up to now, however, no decisive victory has been won by either side, that is, by either the cognitivists or by the non-cognitivists, as we might call them. Arguments are still being presented pro and con, and no settlement of the issue seems to be in sight. Can this be because there are some facts, some bits of evidence bearing on the question, that we have not discovered yet? Is it a deficiency of information that is the barrier to a solution of the problem? That hardly seems likely. Almost every conceivable fact relevant to the question seems to have been mentioned by some philosopher or other. What is much more likely is that the facts have not been correctly interpreted. And one reason for their not having been correctly interpreted is that the question really at issue has not been properly formulated. The question is not: "Are moral judgments cognitive?" The question is something else.

When it is asserted or denied that moral judgments are cognitive, what is meant? What is meant is by no means altogether clear, but we can gather some idea of what is meant from two well-known theses which would generally be regarded as denials of the cognitivity of moral judgments, one that of Carnap, and one that of Ayer. Carnap puts it this way: "Most philosophers have been deceived [by the fact that a value statement has "the grammatical form of an assertive proposition"] into thinking that a value statement [and Carnap would, no doubt, subsume under this term moral judgments] is really an assertive proposition, and must be either true or false. . . . But actually a value statement is nothing else than a command in a misleading grammatical form . . . it is neither true nor false. It does not

assert anything and can neither be proved nor disproved."¹ Ayer argues : when I say that a certain type of action is right or wrong, and somebody else disagrees with me, "there is plainly no sense in asking which of us is in the right. For neither of us is asserting a genuine proposition."² Moral judgments "have no objective validity whatsoever. If a sentence makes no statement at all, there is obviously no sense in asking whether what it says is true or false. And we have seen that sentences which simply express moral judgments do not say anything. They are pure expressions of feeling. . . ."³ From these remarks we may gather that to say that moral judgments *are* cognitive would be to ascribe to them one or a set of the following characteristics :

- (1) they assert something, or they are assertive propositions or genuine propositions, or they make a statement, or they say something ;
- (2) they are either true or false ;
- (3) one can be either in the right or in the wrong about them ;
- (4) they can be either proved or disproved ;
- (5) they are capable of having objective validity.

These characteristics, of course, are not all independent of one another, though their relationships are not in every case clear. To say, for example, that a moral judgment was either true or false would, no doubt, be to imply that it asserted something, or made a statement, or was a genuine proposition, but would it imply that it could be proved or disproved ? Probably not. In any case, we need not try to determine the exact relationship among all these characteristics, because if moral judgments were admitted to have any one of them, they would probably be admitted to be cognitive.

We should note in passing that to hold that moral judgments are cognitive sentences would not be to commit oneself to the view that they express descriptive or empirical propositions. All sentences expressing descriptive propositions are, no doubt, cognitive, but not all cognitive sentences express descriptive propositions. The sentences of mathematics, for example, are cognitive but do not express descriptive propositions. And there may be other sorts of sentences that are cognitive without saying anything descriptive, including moral judgments. Although it

¹ R. Carnap, *Philosophy and Logical Syntax* (Kegan Paul, Trench, Trubner & Co., Ltd., London, 1935), pp. 24-25.

² A. J. Ayer, *Language, Truth and Logic* (Victor Gollancz Ltd., London, 1951), p. 108.

³ *Loc. cit.*

would be difficult to establish the point with anything like certainty, it seems probable that moral theorists of the past would generally have held, had they been aware of the distinction, that moral judgments are cognitive but would have denied that they are descriptive or empirical.

Now, how could one determine whether moral judgments are cognitive or not? From the arguments that have been given in attempts to show that moral judgments are *not* cognitive, it would seem to be believed that what is required to decide this question is some general epistemological theory as to what sorts of cognitive sentences there can be. The non-cognitivists have generally argued that there can be only two kinds of cognitive sentences, those expressing empirical (contingent) propositions, and those expressing analytic propositions; and since moral judgments express neither, according to them, moral judgments are not cognitive. However, this way of going about trying to decide the question of the *cognitiveness* of moral judgments is faced with some serious difficulties. For how could one establish that there are only such and such cognitive sentences? How could one show that there are no kinds of cognitive sentence other than those expressing empirical and analytic propositions? The character of the arguments that have been put forward to establish this is far from clear, but it would seem that there are only two possibilities: either these arguments would have to be *a priori* in some way, or they would have to be based on a survey of what sorts of cognitive sentences there actually are (in which case they could hardly be said to be arguments). Now if the arguments were *a priori*, they would have to involve either synthetic or analytic propositions, but since the non-cognitivists reject the possibility of the synthetic *a priori*, they, presumably, would have to depend on arguments involving analytic propositions. If, however, their arguments depended on analytic propositions, it would be easy to decide whether they were sound, simply by reference to the law of non-contradiction. Yet the fact is that it is not at all easy to decide whether their arguments are sound, because it is not at all clear what is the nature of the statements involved in the arguments. This, in part, must account for the fact that their thesis is still the subject of controversy. In any case, there would be something highly paradoxical in the supposition that one could determine *a priori* what sorts of cognitive sentences there can be. For to determine this one would already have to know that the sentences one used in one's argument were cognitive; but how could one know this without first having arrived at the conclusion of one's argument?

The other possible way is to make a survey of the sorts of cognitive sentences there actually are. How does one do this? Surely only by identifying the sentences that are commonly used by their speakers in order to assert something and that are understood by the persons addressed as asserting something. That any sentence asserts anything, says anything that is true or false, etc., can be found out in no other way than by finding out what it is that the speaker of the sentence means by it, that is, what it is that he intends to convey by it and what it is that he is characteristically understood as conveying by it. If there are any well-established modes of speech that are ordinarily used by speakers with the intention of asserting something, and that are ordinarily understood as asserting something, than it would be absurd to say that they did not really assert anything, but expressed, say, commands. To put it conversely, it would be absurd to say that a well-established mode of speech ordinarily expressed commands if the users of that mode of speech did not ordinarily intend to express commands and were not ordinarily understood as expressing commands. A sentence can express only what it is intended to express. It may not do this very well, in the sense that from it it may, for one reason or another, be difficult for anyone else to gather what it is that is intended to be expressed; and also the speaker may not be clear in his own mind what it is that he intends to express. But if there is any mode of speech that is well-established, that has long been used by plain men for purposes of communication, then one could not reasonably deny that it was at least adequate for expressing what its users intended to express; nor could one reasonably affirm that its users were confused in their own minds about what it was that they wanted to express. The fact that it might be difficult to formulate in other words—other, that is, than the standard mode of speech in question—what that mode of speech was being used to express would not show that it was reasonable to deny or affirm these things. Hence if any standard mode of speech is ordinarily used with the intention of asserting something and is ordinarily understood as asserting something, one could not reasonably deny that it does assert something, that is, that it is cognitive.

If, now, we approach the question of the cognitivity of moral judgments with these considerations in mind, we see that what we ought to be asking about moral judgments, if we want to make clear to ourselves how we should go about finding the answer, is not, "Are moral judgments cognitive?", but rather, "Are moral judgments intended to be cognitive and are they so understood?"

To be specific: Not, "Do moral judgments make assertions?", but rather, "Are moral judgments intended to make assertions and understood as making assertions?" Not, "Are moral judgments true or false?" but, "Are moral judgments intended to say something true or false and understood as saying something true or false?" Not, "Is it possible to be in the right or in the wrong about moral judgments?" but, "Are moral judgments intended to say something about which one can be in the right or in the wrong and are they so understood?" Not, "Can moral judgments be proved or disproved?" but, "Are moral judgments intended to say something which can be proved and disproved, and are they so understood?" Not, "Do moral judgments have objective validity?", but, "Are moral judgments intended to say something which has objective validity and are they so understood?" If the answer to any or all of these questions should turn out to be "Yes", then we must conclude that moral judgments are indeed cognitive. It would not do to say, for example, that though they were ordinarily intended to assert something and were so understood, what they really expressed were commands or feelings. Of course, they might express feelings as well as asserting something; or rather than expressing feelings they might, to use a convenient term of Nowell-Smith's,¹ *contextually imply* that the speaker had a certain feeling.

Now it might be thought—if all this is granted—that it should be easy to find out the answer to our question, "Are moral judgments intended to be cognitive and understood as being cognitive?": all we have to do is ask people whether this is what they intend or understand. But this would not do. Intentions (we should know by now) are not things that we can introspect like toothaches or headaches. We cannot find out what a person's intentions are by asking him to look inside himself and tell us what intentions he sees there. For intentions are not kinds of occurrents but kinds of dispositions. They can be found out only from the way a person behaves or reacts in a variety of situations or circumstances; and the person himself whose intentions they are can find them out in no other way. Somewhat the same is true of understanding. Hence, if we are to discover whether a person intends to assert something by the moral judgments he utters, we must observe the way he utters them, what else he says in relation to them, how others interpret them, and so on; in short, we must observe the characteristic

¹ P. H. Nowell-Smith, *Ethics* (Penguin Books, London, 1954), *passim*.

features of moral discourse, and see how these compare with the characteristic features of discourse already known or, at any rate, believed to be cognitive. And let us proceed to do that now.

One of the most striking features of moral judgments is that they take the form of indicative sentences: *e.g.*, "Lying is wrong", "One ought to keep one's promises", "She is a virtuous woman", "The sentence was unjust", and so on. It is this fact, it has been alleged (for example, by Carnap, in the passage quoted above), which has misled various philosophers to conclude that moral judgments are cognitive, whereas in fact (so it is said) they are imperative or emotive. Now if this fact could mislead philosophers to conclude that moral judgments are cognitive, it must surely be because it is a characteristic feature of sentences known to be cognitive that they take the form of indicative sentences; otherwise, the fact that moral judgments take the form of indicative sentences would scarcely lead or mislead anyone to conclude that they were cognitive. If, then, moral judgments share this feature of being indicative with sentences known to be cognitive, may we not take that as some evidence that moral judgments are intended by their speakers to say something? For if moral judgments are not intended to say or assert something, if they are really intended to be commands or interjections, why do they not take the form of commands or interjections? Moral judgments are like assertions; they seem to be intended to assert something; and if those who utter them do not really intend to assert something, why do they adopt a form of words that gives the impression that they do? One could scarcely argue that the assertive form of words that moral judgments take is merely an accident of our language: for it is also the form of words that they take in French, German, Latin, Greek, and, I suppose, in other Indo-European languages; and in at least one non-Indo-European language, Hungarian, and, no doubt, in a good many others as well. Perhaps, then, ordinary people who utter moral judgments have been misled in the same way as some of those wiser than themselves, some philosophers, have been misled? But no, that would not make sense. Ordinary people could not have been misled by the assertive form of moral judgments into thinking that moral judgments were cognitive and so have been led to adopt the assertive form for moral judgments. But could not ordinary people have thought that they were intending to say something cognitive when in fact they were intending to say something non-cognitive? Then at least moral judgments would appropriately reflect what people thought they were intending to say, and an analysis of moral

judgments would still have to be made in cognitive terms in order correctly to express what people thought they were intending to say when they uttered moral judgments. However, the notion that people thought that they were intending to say something cognitive while in fact they really intended to say something else, and that is why moral judgments took the form of assertive sentences rather than of, say, commands, is so implausible as not to be worthy of more than passing mention.

The indicative or assertive form of moral judgments is a fact, then, which cannot simply be dismissed as a grammatical peculiarity of no importance—if our question is, not “Do moral judgments assert something?”, but rather, “Are moral judgments intended to assert something?” If it is to be dismissed, it must first be explained, or explained away. But perhaps this could be done, after all, if moral discourse took the form only of moral judgments, that is, of subject-predicate sentences with a moral term in the predicate which seems to be asserted of the subject. There do seem to be some sentences in the indicative which are imperatives: e.g., “In future you will no longer see her”, said by an angry father to his son with the object of preventing an unsuitable marriage.¹ Could not moral judgments be like these sentences? Perhaps; at any rate it would not require too strained an interpretation of them if moral discourse took the form only of moral judgments. But in fact moral discourse abounds in sentences that are not moral judgments at all, sentences that we might call *non-judgmental moral sentences*, where “moral sentence” means the same as “sentence in moral discourse”: and whatever we say about moral judgments must apply, with suitable but minimal changes, to non-judgmental moral sentences.

There are, for example, moral questions: “Is it always wrong to tell a lie?” “Is the judge just?” “Can it ever be one’s duty to sacrifice one’s family for the sake of one’s country?” and so on. Now this form of words seems to indicate that the speaker is asking for some sort of answer that can be true or false, or correct or mistaken, or have objective validity. It is a form of words that is peculiarly characteristic of cognitive discourse generally. If moral judgments were not understood as saying something cognitive but rather something imperative or emotive, this form of words would be, to say the least, highly inappropriate. At any rate, as in the previous case, a rejection of this form of words as indicative of the speaker’s understanding moral judgments to be

¹ But could not this be regarded as one alternant of a disjunctive sentence in which the other alternant is taken to be understood—“or I will cut you off without a penny”, for example?

asserting something would, in order to be plausible at all, have to be supplemented by an explanation of how this disregard of words could have gained currency. But the explanation could not be, surely, that plain men were misled by the indicative form which plain men used to express moral judgments into thinking that moral judgments asserted something, and so came to think that they could turn moral judgments into questions.

Another type of non-judgmental moral sentence is that in which a moral judgment appears in indirect discourse as the object of what may be called a "cognitional" verb or expression, such as "know", "believe", "think", "be of the opinion", "come to the conclusion", etc. Thus we very commonly say things like "I knew it was wrong, but I couldn't resist the temptation", "He is too young yet to know that it is wrong to take something that doesn't belong to one", "Baptists believe that drinking alcoholic beverages is wrong", "He is now convinced that divorce is not wrong", "He came to the conclusion that it was his duty, after all, to reveal the information", "I am inclined to think that it would not be right to interfere, but I can't make up my mind about it". Immediately related to these modes of speech are such sentences as: "He is a man of firm moral convictions", "He acquired his moral beliefs from his mother", "The moral opinions of plain men are not really very different from those of philosophers". In these sentences moral judgments are indirectly referred to, rather than indirectly reported, as objects of cognitions, for what is meant by a man's "moral convictions", for example, is his conviction that such and such things are right, or wrong, or something else of the sort. Now, what are we to make of sentences like these? I should think we could regard them as pretty clear evidence that when ordinary people utter moral judgments they intend to say something cognitive and are so understood. Moral judgments, these sentences indicate, are forms of words expressing things which those who utter them and hear them regard as possible objects of knowledge, belief, conviction, doubt, and ignorance. If what moral judgments were intended to express were commands or feelings or attitudes, rather than cognitions, how could it have come about that they should quite commonly be used as objects of cognitional verbs? On the imperative or emotive theory of moral judgments, such sentences as "I know it's wrong to lie" or "He believes that divorce is not wrong", must either make no sense and ought not to be used at all, or be given a strained *ad hoc* interpretation to make them at least not inconsistent with the theory. What seems paradoxical to the plain man (to this plain man, at any

rate) about the non-cognitive interpretation of moral judgments is that on that interpretation one could say to someone who said, for example, "I know (believe) it's wrong to tell a lie", "Well, of course, you can't really *know* (believe) that it's wrong to tell a lie, because the words 'It's wrong to tell a lie' don't express anything that *could* be known (believed), since they express commands or feelings. Therefore, strictly speaking, you ought not to say you *know* (believe) it's wrong to tell a lie; or else what you know (believe) is not that *it's wrong to tell a lie*, since this, as I have just pointed out, doesn't say anything that could be known, but something else, such as that most people disapprove of telling lies, or I disapprove of it—something like that." This seems paradoxical to the plain man because it never occurred to him that he was not speaking strictly when he said that he knew it was wrong to tell a lie, or that he knew anything else but that it was wrong to tell a lie. It seems paradoxical because—and this is the simplest explanation of it—the plain man really intends to assert that it's wrong to tell a lie when he says "It's wrong to tell a lie".

There is, moreover, further evidence on this point. For in ordinary moral discourse it is quite appropriate to apply the terms "true" or "false", "correct" or "mistaken", or their equivalents (e.g. "it is so", "it isn't so") to moral judgments: "I don't think it's true that people ought never to tell lies"; "He argued that it was my duty to return the money, but I was able to convince him that he was mistaken"; "I don't think it would be correct to hold that the war was unjust, but it was certainly unwise of them to start it". These cognitional appraisal terms, as we might call them, seem quite clearly to be used in their ordinary senses in these examples, but they could not be so used if the moral judgments they were used to appraise were not intended to assert something. One may, if one pleases, argue that moral judgments cannot be true or false, correct or mistaken; but the fact remains that in ordinary moral discourse these epithets are applied to moral judgments, and their use in this way does not at all seem to be Pickwickian. This fact surely indicates that in ordinary discourse moral judgments are intended to convey cognitive meaning. How else could we account for it? One might say that ordinary people, just as philosophers, have been misled by the indicative form of moral judgments into thinking that they were saying something cognitive when they uttered moral judgments, and so felt free to treat moral judgments as they did other cognitive sentences, including making them objects of cognitional verbs and applying cognitional appraisal

terms to them. But if ordinary users of moral speech have been so misled by the form of the moral judgment, that form must be singularly inept for conveying the meaning which people who use it want to convey : for not only does it systematically mislead those to whom it is addressed, but also those who use it, so that *both* think that something is being said which is not really intended to be said. And that, I suggest, is absurd.

There are still other highly characteristic features that moral discourse shares with cognitive discourse generally and that could not have been predicted if what moral judgments were intended to express were merely feelings or attitudes, or if moral judgments were really imperatives. The fact that these features of moral discourse could not have been predicted on the basis of the emotive or imperative theories of moral judgments means that each one of them would have to be given an *ad hoc* explanation by an adherent of those theories. But the necessity for *ad hoc* explanations is a weakness in a theory and a good reason for holding it suspect.

One additional feature of moral judgments is that moral judgments have an impersonal character and objectivity is expected of those who make them. If we believe that someone who utters a moral judgment is influenced by his personal desires, or by his emotions, or by his prejudices, we tend to discount it, just as we do in the case of other sentences intended to be cognitive. "He maintains that the sentence is unjust, but that is only because it bears heavily on him." A : "You say that dancing is wrong. But why ? Simply because you have been led to disapprove of it by your puritanical parents. The fact, however, that you or anyone else disapproves of it doesn't make it wrong." To this B may reply : "I am not contending that it does ; and while it is true that I first learned to disapprove of dancing from my parents, I have since then arrived at the conclusion that it is wrong quite independently of their influence." "They maintain that we have a duty to fight the French. But have we ? Aren't they simply saying that because they would stand to make enormous profits from a war right now ?" Examples such as these—and many more could be given—show that if we are to take someone's moral judgment seriously, then, *even if* moral judgments express feelings or attitudes, we must believe that they are not simply the attitudes of the speaker, but represent some general, impersonal standpoint. If we are to take, say, B's judgment that dancing is wrong seriously, then, even if B's judgment is the expression of an attitude of disapproval, we must believe that it is the expression, not of B's attitude *qua* B's, but of B's attitude *qua*

unprejudiced, disinterested, and possibly reasonable, well-informed, etc. And if the judgment is understood as an expression of disapproval of an agent *qua* unprejudiced, disinterested, etc., that is tantamount to its being understood as saying that an unprejudiced, disinterested, etc., agent would disapprove in so far as he was unprejudiced, disinterested, etc. And that would be tantamount to making an assertion, an assertion that was true or false, about which one would be in the right or in the wrong, and which was capable of having objective validity. Hence, on any plausible theory of moral judgments' being the expression of feelings or attitudes, they must be regarded as the expression of the feelings or attitudes of an agent *qua* ideal in some way, which would make them tantamount to assertions about what would be the feelings or attitudes of an ideal agent—which would make them cognitive sentences!¹

The second additional fact I wish to point out is the use of abstract nouns derived from the moral predicates, such as "rightness", "goodness", "virtue", "justice", etc. In ordinary moral discourse there quite commonly occur such non-judgmental moral sentences as the following: "Virtue is its own reward"; "I am not questioning the prudence of the action, but its rightness"; "Many men have sacrificed their lives for the sake of justice"; "Goodness is a quality all too rarely found among men". Sentences such as these could not have been predicted on the imperative or emotive theories, and at best, could be given only a painfully strained interpretation by the proponents of these theories. What they seem clearly to indicate is that plain men who engage in moral discourse intend to be understood as talking about certain properties when they use the words "right", "just", "virtuous", etc. If they used these words simply as vehicles for, say, expressing feelings or attitudes, there would be no reason to make abstract nouns out of them, and such abstract

¹ Ideal agent or, as they are more commonly called, ideal spectator or observer theories have been suggested or advanced by Hume (according to Rachael M. Kydd), Adam Smith, C. D. Broad, F. C. Sharp, Roderick Firth, Richard Brandt, and possibly others. They are by far the most promising of the attitude theories, yet they have received relatively little attention. See: Rachael M. Kydd, *Reason and Conduct in Hume's Treatise* (Geoffrey Cumberlege, London (1946), p. 175. Adam Smith, *The Theory of Moral Sentiments*. C. D. Broad, "Some Reflections on Moral-Sense Theories in Ethics". *Proceedings of the Aristotelian Society*, N.S., vol. xlv (1944-45), 131-166. F. C. Sharp, *Good Will and Ill Will* (The University of Chicago Press, 1950), p. 162. Roderick Firth, "Ethical Absolutism and the Ideal Observer", *Philosophy and Phenomenological Research*, vol. 12 (1951-52), 317-345. R. B. Brandt, "The Status of Empirical Assertion Theories in Ethics", *MIND*, lxi, (1952), 458-479.

nouns could have no intelligible sense. But in fact these abstract nouns are used in ordinary discourse much like other abstract nouns known to stand for properties, and observation of discussions involving the use of these nouns reveals no evidence that the participants intend to use them or understand them in any other sense than as the names of properties. Some philosophers, however, find it difficult to believe that these terms can stand for properties, for at least two reasons: (a) when they examine acts or characters said to be right or wrong, good or bad, etc., they cannot find any properties for which these words might stand; (b) they are convinced on other grounds that moral judgments must somehow be defined in terms of attitudes—that unless human beings were capable of attitudes such as approval or disapproval, there would not be such a thing as a moral judgment; and if moral judgments must be defined in terms of attitudes, they could not have objectivity, which, however, is what discourse about properties would have to have. Now neither of these reasons is a good reason for rejecting the possibility that “rightness”, “goodness”, etc., could stand for properties. (a) Those who make this rejection on the ground that they cannot find any such properties when they examine the things said to have them fail to draw a distinction between intrinsic properties on the one hand, and relational and dispositional properties on the other. Intrinsic properties are those that can be found by examining the thing itself, such as the colour of a book, or the size of a table. But there are many property names that do not stand for properties that could be found simply by examining the thing itself, but for properties definable only in terms of the relation of the thing to other things: either an actual relation, in which case we may call the property a “relational” property (e.g. “famous”, “rich”), or a relation which would obtain under certain conditions, in which case we call the property a “dispositional” property (e.g. “soluble”, “intelligent”). These properties could not be found in the thing that had them if one examined only that thing by itself. Now it is quite possible that moral terms stand for such relational or dispositional properties rather than for intrinsic properties; indeed, they *must* stand for such properties if they stand for properties at all, since it is quite true that they cannot be found merely by examining the acts or characters said to have them. (b) Even if we grant that moral judgments must be definable in terms of attitudes, say of approval and disapproval, we are not compelled to deny objectivity to them. For it is possible to suggest a dispositional analysis of moral judgments, such as those of Hume, Adam

Smith, Sharp, Broad, Firth, and Brandt, according to which they would be defined in terms of attitudes and yet be perfectly objective. Thus we might say that "Lying is wrong" means the same as "Lying is such that it would be disapproved of by an impartial, intelligent being who knew the consequences of lying". On this analysis, the property of wrongness attributed to lying could not be discovered simply by examining instances of lying; it would be a property defined in terms of an attitude of disapproval; and yet the judgment would be, not only cognitive, but perfectly objective in the sense that it could be true of lying no matter what attitude people might actually have towards lying. But in any case, the fact that abstract nouns derived from the moral predicates are a standard feature of ordinary moral discourse is a pretty clear indication that plain men intend to be talking about properties when they use such words as "right", "wrong", "good", "bad", etc., however difficult we may find the task of defining these properties. And if plain men intend to be talking about properties when they use such words, they intend to make assertions about them, *i.e.* to utter cognitive sentences.

The third additional feature of moral discourse I wish to direct attention to is the occurrence of moral judgment or debate. We quite commonly encounter such dialogues as the following:

- A. "I contend that lying is wrong under all circumstances."
- B. "No, I think you are mistaken there. Under certain circumstances lying is fully justified. Suppose that by telling the truth you should expose hundreds of persons to misery; surely you would not maintain that then one ought to tell the truth."
- A. "No, but one could keep silent."
- B. "Ah, but suppose you had no choice: either tell the truth or lie."
- A. "In that case I should still maintain that lying was wrong, because to admit that under certain circumstances lying was justified would result in far greater misery for everyone in the long run than could befall these people."

And so on. Now the proponents of the emotive theory have said that (a) when people talk in this way, they are not really disagreeing in belief, they are merely disagreeing in attitude, so that they are not really arguing, but simply trying to change one another's attitudes; or that (b) if people do disagree in belief, what they disagree on are the empirical facts of the matter and not the alleged truth of any moral judgment. Let us consider (a). That when people seem to be engaged in moral debates they

are not really disagreeing in belief but only trying to influence one another's attitudes is a view that could hardly have suggested itself to anyone who considered the features of such debates from an impartial standpoint, independently of a previous commitment to some non-cognitivist theory of the moral judgment. Moral debates make use of all the characteristic modes of speech that one finds in debates where there is disagreement of belief; these modes of speech are not at all what one could have anticipated if the debates expressed merely disagreements in attitude. The participants in such debates talk as if what they intended to express were assertions and as if they understood their opponents to be making assertions. Are they, then, themselves deceived? Do they think they intend to make assertions when what they really intend is to express attitudes or alter those of their opponents? I have already urged that an affirmative answer to such questions would be absurd. The thesis that moral debates express disagreement in attitude and not in belief is an *ad hoc* assumption that has to be made by anyone who has decided prior to a survey of the characteristic features of moral discourse that moral judgments are not assertions. If, however, one were simply trying to determine what it was that the participants in a moral debate were intending to express and what they understood one another as expressing, the conclusion that one would naturally come to would be that they were trying to prove or disprove some assertion. And this conclusion would be confirmed by the other modes of speech that we find in moral discourse, all of which are such as one would expect if the persons who used them intended to assert something when they uttered moral judgments; and none of which are such as one would expect if by the moral judgments they uttered they intended merely to express feelings or attitudes.

As to (b), the contention that people who engage in moral debate disagree only on what the empirical facts of the matter are and not the truth of some moral judgment. I am inclined to agree that if one examined moral debates one would find that what it was that the participants of such debates actually disagreed on was in each case some matter of empirical fact. But it would not follow from this that the moral judgment the disagreement concerning which occasioned the debate was not intended and understood to be saying something true or false. For it may well be that the truth of the moral judgment depends on the empirical facts in some way, so that when the empirical facts are known, the truth of the given moral judgment would immediately follow. Suppose, for example, that the moral predicate "right"

stood for a dispositional property such that any act that had a certain set of intrinsic, relational or dispositional empirical properties would have the further dispositional property signified by the word "right". Then what we would have to do to decide whether an act was right or not would be to decide whether it had that set of intrinsic, relational or dispositional empirical properties; and that might be very difficult to decide and might well occasion prolonged or even interminable debate—debate about what the empirical facts of the matter were. But the debate would still be undertaken with a view to determining the truth or falsity of a moral judgment, the judgment that the act in question is right or not. Hence, even if it is true that when people engage in moral debate what they are actually disagreeing on are the empirical facts of the case, it does not follow that their disagreement concerning the moral judgment which occasioned the debate is a disagreement merely in attitude and not a disagreement concerning the truth or falsity of an assertion.

Let us now summarize. If we ask, not "Do moral judgments assert something?" but, "Are moral judgments intended to assert something and are they so understood?", we must admit that moral discourse bears every evidence that those who engage in it do intend to assert something when they utter moral judgments and are understood as asserting something. The fact that moral judgments are expressed in the indicative mood, which is the standard assertive form of discourse; that they can be re-worded as questions; that they can be objects of cognitive verbs; that cognitive appraisal terms are applied to them; that they are impersonal and objectivity is expected of those who utter them; that their predicates often take the form of abstract nouns; and that disagreements concerning them occasion debates which sound just like debates concerning any other type of assertion: all these facts are just those that one would expect if moral judgments were intended to be assertions, capable of being true or false, capable of being objective, about which one could be in the right or in the wrong, and which could be proved or disproved. *Moral discourse is redolent*, so to speak, *of cognitivism*. If, now, an emotivist, for example, should say to us, "But moral judgments express neither empirical nor analytic propositions, and so I just can't understand what they could express other than feelings or attitudes", we could reply, "Well, that's a pity, isn't it? But you see, my dear fellow, when we plain men engage in moral discourse we make it abundantly clear by the various modes of speech we employ that we really

do intend to be making assertions when we utter moral judgments; and the plain men whom we address seem to take that to be our intention, too. So your interpretation of our moral judgments as being nothing more than expressions of our feelings or attitudes just doesn't fit the facts, does it? It is certainly true that we cannot tell you in other words what we intend to assert by our moral judgments; but that's what you philosophers are here for, to find out."

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In No. 244 of MIND 1952 a drawing of G. E. Moore was published on the occasion of his being awarded the O.M. The artist was H. Andrew Freeth. The Publishers of MIND still have a limited stock of these. Unmounted copies cost £3 3s., mounted £3 12s. 6d. Orders for these should be sent to Miss J. Gow, Subscriptions Dept., T. Nelson & Sons, Ltd., Parkside Works, Edinburgh 9.

VII.—DISCUSSIONS

FRANKENA ON OUGHT AND CAN

FRANKENA limits his discussion to what he calls 'the ethical senses' of 'ought' (p. 157).¹ 'Can' is taken in the sense, 'I am actively able to do A, if I choose' (p. 159). He also limits his analysis to certain types of ethical norms. Furthermore, his analysis is based on the assumption that ought-statements can be true or false and does not bring in the problems which evolve if we accept that ought- and can-statements may fulfil non-cognitive functions, e.g. certain emotive functions. In this critical examination of his argumentation I shall limit my analysis in a similar way.

The main purpose of my paper is to try to show that Frankena has not clearly distinguished between the following questions: (a) "Do certain people use 'ought', in this or that situation, in such a way that 'can' is implied?", (b) "Ought an ethically reasonable and consistently thinking person to use 'ought', in this or that situation, in such a way that 'can' is implied?" In order to answer the first question we must analyse the actual usage of certain persons. The analysis fulfils what we might call 'the midwife-function'. Answers to the second question usually take the same form as answers to the first. The author says, e.g. "Hence, 'ought' in this context implies 'can'", whereas the statement rather should have been expressed this way: "Hence, according to the ethical system E, 'ought' in this context *ought* to be interpreted in such a way that it implies 'can'." Analysis of this kind fulfils what we might call 'the missionary-function'. My main complaint may be expressed, therefore, by saying that Frankena's analysis does not clearly distinguish between the midwife-function and the missionary-function of analysis; a failure which seems to me to be rather typical of modern analytic metaethics.

Statements saying that a person ought to feel so and so

Frankena wants to show that in actual usage there is a sense of 'ought' which does not imply 'can' (p. 160). He tries to show this by first pointing to the fact that we admittedly apply 'ought' to feelings. For example, we say that a person ought to feel gratitude towards a benefactor. Secondly, he points out that a man cannot, here and now, by any effort of will create within himself a feeling of gratitude. He cannot, for instance, feel gratitude in that sense of 'can' that Frankena is discussing. From these two considerations he concludes that 'ought' does not here imply 'can'.

¹ All references are to W. F. 'Obligation and Ability', printed in *Philosophical Analysis*, ed. M. Elack, Ithaca, 1950.

This inference seems to me incorrect. We can only infer that some people in some situations utter: 'P ought x' about something which—we take that for granted—P cannot accomplish in Frankena's sense of 'can'. Why does Frankena believe that the people whose usage of 'ought' he investigates, believe that we cannot feel gratitude by an effort of will? What is his evidence for believing that their use of 'ought' has been *influenced* by their holding that we cannot feel gratitude at will—if we suppose that they *do* hold that?

Statements of the kind 'You ought to feel so and so' may express aggression. To interpret people's usage so that 'ought' does not imply 'can' may be to rob the statement of its point. To propose, as an analyst, an interpretation of 'ought' which excludes 'can', is not so much a help in the clarification of a linguistic matter, as it is an indirect attempt to make the statements more reasonable.¹

The lack of sufficient distinction on this point is indicated through the following statement: "It is more plausible to hold that rules of actions use 'ought' in a sense implying 'can, if I choose' than that rules of feelings do so, for actions seem to be in the control of the will in a way in which feelings are not" (p. 161). The hypothesis which Frankena says that he is trying to substantiate is concerned with actual usage. But the argument given in favour of the hypothesis does not display observations of actual usage. The argument is that feelings are not in the control of the will. Even if this is true and even if the presumed users of 'ought' do in fact consider it true, it is still an open question whether this belief has influenced their use of the term.

Statements about prima facie duties of action

Frankena next turns to statements asserting *prima facie* duties of action. The statement, 'We ought to keep our promises' may be interpreted as stating such a duty, for instance, that the keeping of promises is our actual duty only in those circumstances where no other moral features of the situation prevent its being an actual duty (p. 162).

His conclusion is that statements of this type do not imply 'can, if I choose' (p. 163). His main argument is this: if we suppose that such *prima facie* rules enjoin us to *do* certain actions, and not merely to *try* to do them, then 'ought' cannot imply 'can', since it is not always the case that we can do a *prima facie* duty if we choose.

Let us grant that Frankena is correct in holding that we cannot always do a *prima facie* duty, even if we choose to. How can we from that statement about people's abilities infer anything about their use of 'ought'? Here, as above, we must ask how Frankena can know that the users of 'ought' share his opinion on the extent of man's abilities. Unless Frankena can be sure that they do, he

¹ Cf. footnote on p. 77.

has no guarantee that his conclusion, which is based on that premise, will hold for their usage. The fact that he has not taken up this question indicates that he is only half-heartedly interested in people's actual usage. His conclusions as to whether 'ought' implies 'can' are not so much conclusions about the relationship which exists in people's usage, as they are statements about the relationship which *would* exist, if the people whose usage he is describing had certain specific opinions on man's abilities, and if they accepted certain norms according to which their ought-statements ought, or ought not, to imply these opinions.

Statements about actual duties

Frankena then turns to statements about *actual* duties. He mentions as an example the duty to do the act which actually fulfils most fully one's *prima facie* duties. For such statements Frankena concludes that 'ought' implies 'can, if I choose' (pp. 166-167). He calls this use 'ought (2)'. The 'ought' which does not imply 'can, if I choose', he calls 'ought (1)'.

As an indirect support of his hypothesis that 'ought (2)' implies 'can' he quotes Carritt, saying: "I think it would be improper to speak of an obligation we *could* not fulfil if we tried, such as to move in two directions at once or to learn Chinese in a week." What is Carritt's criterion of proper use? Does he mean that it would be bad English, or that it would be logically inconsistent or that it would be ethically improper?

No matter which of these alternatives gets Frankena's support, it seems that none of them, according to his view, should have it. For some pages above (p. 160) he has approved a quotation from Ewing, saying that the criterion of proper use can only be actual usage. Consequently, Frankena should only ask a single question: "Do these or those people use statements of the kind, 'The person P ought to do what best fulfils his *prima facie* obligations' in such a way that it is implied that P can so act, if he chooses?" Hypotheses about our abilities or our beliefs about our abilities are relevant only in so far as they throw light on this question of language.

Statements saying that we ought to do what probably will have this or that result

Frankena maintains that we use 'ought' also in a third sense, e.g. in sentences saying that one ought to do the action which probably will be most beneficial. Frankena's hypothesis is that this usage does not imply 'I can, if I choose', but only 'I probably can, if I choose'. This hypothesis too, seems to pretend validity for actual usage.

His argument is that I can no more know, except with probability, what I can do, than what is my objective duty. Even if this is granted, it gives no support for his conclusion. What follows from the statement is only this: if 'ought' is used to imply 'can, if I choose', then the ought-statements in question assert something

which the assertor cannot know to be true. Why does Frankena believe that we do not use the ought-statements with this implication? We often assert something which we do not know to be true. Even if Frankena had shown that the ought-statement by implying 'can, if I choose' implied a false statement, it might be that so-called common usage implied this false statement. In that case an analysis of usage, aiming at fulfilling the midwife-function, should bring this out explicitly.

Statements saying that P ought to do what he thinks he ought to do

Frankena distinguishes finally what he considers to be the fourth sense of 'ought', the sense according to which a person ought to do what he thinks he ought to do, in the situation as he perceives it. As regards this use of 'ought' he maintains that it entails 'I think I can do it, and I can try to do it' (pp. 171-172). Frankena tries to show why ought-statements of this type only entail 'can try to do it, if one chooses', and not 'can do it, if one chooses'. His reason is that this type of ought-statements do not, according to him, ask us to do anything. They ask us only to *try* to do it.

Frankena tries to support his position by asserting that this interpretation of the ought-statements is the only way out of a difficulty, which he calls 'incredible' (p. 171). What is this difficulty? It is this: suppose that the ought-statements in question ask us to do something, not only to *try* to do it. In that case it follows, according to Frankena, that we ought to do something, even if we cannot do it, and this, to him, seems an unacceptable consequence.

Frankena's reasoning does not seem quite clear on this point, and I shall go through the problem in more detail. First I shall try to show that 'ought (4)'-statements do not necessarily imply 'P thinks he can'.

Let us start with a statement using 'ought' with the fourth sense: 'P *ought* to adopt that course of action which he thinks he ought to adopt in the situation as he perceives it.' In this statement it is '*ought*' not 'ought', that Frankena asserts is used with the fourth sense. 'Ought', he says, is used with the second sense, i.e. with the sense in which we use 'ought' when we say that one ought to do that act which best fulfils one's *prima facie* obligations. This second sense of 'ought' Frankena calls 'the objective ought'.

Here is an example of how such an ought (4)-statement may look when its entailment is made explicit: 'P *ought* to adopt that course of action in the situation S, as he perceives this situation, which he thinks he objectively ought to do'. This 'ought (2)' implies, according to Frankena, that one 'can do the act, if one chooses'. However, Frankena does not say that 'ought (2)' entails 'can'. But perhaps that is his intention. In that case still more words may be smuggled out of the statement above. In its new interpretation it says: 'P *ought* to adopt that course of action in the

situation S, as he perceives this situation, which he thinks he objectively ought to do, and which he therefore also thinks he can do, if he chooses.' On the other hand, if he maintains that 'ought (2)' implies, but does not entail, 'can, if one chooses', then it does not follow that a person thinking he ought (2) to do x, also thinks he can do x, if he chooses. From the fact that he thinks he ought (2) to do x, nothing at all follows as to what he *thinks* on the latter issue. Suppose 'P thinks that he objectively ought to do x' may be interpreted to mean that P believes that it is true that he ought (2) to do x. According to the view above 'P ought (2) to do x' *implies*, but does *not entail* 'P can do x, if he chooses'. If P actually cannot do x, if he chooses, then the consequent of this implication is false, and the implication is true only if the antecedent is false. Hence, on these suppositions, P is holding as true a statement ('I ought to do x'), which is false. But it does not follow that he thinks he can do x, if he chooses.

In both interpretations of the 'ought (4)'-statement, 'ought (2)' occur as part of a *description* of a certain action. According to the first interpretation, the following is true: if there is no action which P thinks he can do, if he chooses, there can be no action which he thinks he ought (2) to do, and consequently there is nothing which P ought (4) to do. According to the second, and more plausible interpretation, it does not follow from the occurrence of 'ought (2)' in the 'ought (4)'-statement that P is free from his 'ought (4)'-obligation to do x in case he thinks he cannot do x.

However, so far nothing has been said about the possible implications of 'ought (4)'. We have discussed only the implications of 'ought (2)' occurring in the 'ought (4)'-statement. 'P ought (4) to do x' may be interpreted as elliptical for 'P ought to do, and can do that act which he thinks that he ought to do in the situation, as he perceives it.' If this 'ought (4)'-statement is true, then P can do the act in question. The statement does not imply, as Frankena seems to think, that P ought to do x, even if he cannot. If P cannot do x, then the 'ought (4)'-statement is false. Furthermore, 'P ought (4) to do x' may also be interpreted as elliptical for 'P ought to do, *if he can*, that act which . . .'. Hence, I cannot see that Frankena has shown that to interpret 'ought (4)'-statements as saying that we ought to *do* leads to incredible results, and that we *therefore* must interpret them as saying that we ought to *try* to do.¹

¹ In connection with the question whether a certain analysis of an ought-statement leads to incredible results the following should be mentioned: If we consider ought-statements capable of being true or false, then an ought-statement which entails 'can' may be milder than one which does not. In the former case, saying 'P ought to do x' entails 'P can do x'. If P cannot do x, then the ought-statement is false, and P is not obligated. On the other hand, if we consider ought-statements as non-cognitive norms or prescriptions, then an ought-statement which entails 'can' may be *more severe* than one which does not. Saying to a person 'You *ought* to do x' may, according to this view, be to subject him to some kind of pressure.

Let us, however, accept that the analysis in question leads to incredible results. Why does Frankena believe that the people whose usage he analyzes is aware of it and that they have already taken care to avoid it? For if they have not avoided the difficulty, Frankena's analysis is wrong judged as an analysis of their usage.

Let us now, for the sake of argument, accept that 'ought (4)'-statements are interpretable as statements saying that we ought to try to do, rather than that we ought to do.

On this interpretation the statement we have been concerned with gets the following form: 'P ought to try to do that course of action in the situation S, as he perceives this situation, which he thinks he objectively ought to do.' If we apply to this statement the same type of reasoning which Frankena applied to it in its earlier form (when it asked us to do), then it implies that P ought to try to do the act, even if he cannot try, if he chooses. Are there any reasons to consider this result as less incredible? Well, that depends upon the meaning of 'try'. If this term is interpreted so leniently that one is said to have tried, even if one has only whispered the words 'I am trying', then the result is not very incredible. One can fulfil one's obligation by a whisper only. If the term 'try' is given a more strict meaning, however, it sometimes may be false to assert that a person can try to do what he thinks he ought to do.

The usage of the ethically reasonable man

Although Frankena says that he is trying to clarify common usage (e.g. p. 175), he seems more concerned with making suggestions for the relationship between 'ought' and 'can' in the usage of what we might call 'the ethically reasonable man'—a myth of analytic ethics, comparable to the myth of the economic man in economics. He is not describing the usage of this man, he is helping to create it. The ethically reasonable man is not a man whose usage could be identically described by several independent analysts. He is an ideal. This ideal sets certain vague limitations for the creative activity of the analyst in what he calls 'analysis of actual usage'. Once in a while an analyst is able to add a feature of the ideal which almost all analysts think good. From that time on, the work in completing the ideal is limited a bit more.

Different ethical systems

Frankena's analysis seems to proceed on the assumption that there is one, and only one, correct ethical system. This assumption may perhaps be fruitful in certain connections. But it is not useful

If the statement even says 'You can do x', the pressure may thereby be increased. And the fact that P cannot do x, does not make the ought-statement false. It may be upheld even in this case. Frankena's reasoning seems to me mostly based on a cognitive theory of ought-statements, but in some cases he seems to come closer to a non-cognitive theory.

as a means to mapping out the ethical systems accepted by living people and real enough to influence their actual use of 'ought'. One example of this must suffice. Frankena points out that some authors have defined 'right act' as the one which probably will be most beneficial. He says: "We have here, in fact, a family of possible senses of 'ought' . . . and which all involve the notion of what probably ought to be done . . ." (p. 168). This family is different from the family of senses which define 'right act' as the one which will actually be most beneficial. My question now is: "Why does Frankena believe that we are here confronted with different meanings of 'ought'?" If he is correct in this, the result will be that people whom we think are disagreeing, will not be disagreeing. Let us suppose that A says that P ought to do what probably will be most beneficial, and B answers that P ought to do what actually will be most beneficial. On Frankena's view A and B are not contradicting each other, since they are using 'ought' in different senses. This may be a possibility, but I think it is unjustified to overlook the fact that they may be disagreeing on a normative question.

The relation between 'ought' and 'can' is relative to ethical systems

Frankena tries to answer the question of the relation between obligation and ability by analysing ought-expressions. But the question whether a certain ought-formulation implies 'can' cannot in most cases be answered by an analysis of 'ought'. It can be found out only by an analysis of the ethical system in which the ought-statement is incorporated. Only by this kind of analysis is it possible to discover the conditions that must be fulfilled in order that a certain ought-statement shall be applicable. It is only in very rare cases that these conditions are expressed by use of the term 'ought'.

Rather than studying ought-expressions, a more fruitful approach seems to be this: First of all we decide on which ethical system is to be studied. It may be that of a particular person or the system expressed in a certain publication, etc. Within this system we select one norm at a time, e.g. the norm N, which probably will have to be interpreted in different ways. N says, e.g. that P ought to do the action A. The question is now: 'Is it plausible to interpret N, or any other part of this system, so that it says that P can do A or can try to do A, etc., or so that it asserts that P ought to do A only on the condition that P is able to do it?' The next question is: "In which sense of 'can' is it stated that P can do so and so?" When the problem is stated this way, we see that the ethical system may include conditions regarding P's ability without expressing them by the word 'ought', and that it is up to the system itself to decide whether it will consider a person free from his obligations in case he cannot fulfil them.

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TRUTH AS APPRAISAL

NOT surprisingly, philosophers who have grown tired of backing the classical trinity for the Theory of Truth are looking to other stables for winners. So far, however, the new favourites, though promising on form, have proved disappointing in the matter of staying power. We hear complaints that the performative horse has already been ridden further than he will go. And I want to suggest that the evaluative horse is an even worse bet; for Mr. A. R. White ("Truth as Appraisal", *MIND*, lxvi (July 1957) pp. 318-330) has backed a non-starter.

White summarises his view as follows:

My thesis is that the function of the word 'true' (or 'false') is to *appraise* whatever it is used of, whether statement or object. Like all appraisal words, it has two aspects, a descriptive and an evaluative (p. 318).

I have two main objections to this, which I summarise as follows. (1) There is no evaluative aspect of truth, in the sense required—the sense, that is, in which 'true' would be like 'good'—because 'true' and 'false' are polar opposites and do not admit of degrees. (2) Even if a case could be made out for non-scalar evaluations, yet it has not been shown that any such "pro-attitudes" are involved in standard contexts of 'true', White's thesis borrowing a superficial plausibility from the deliberate assimilation of *statements* to *objects*. White is aware of (1) and takes steps, quite unsuccessfully I think, to meet it. He does not seem to be uneasy about (2), merely noting that cases where 'true' and 'false' are used of objects rather than statements (as in 'true friend', 'false nose') are "not usually considered". I shall take this second objection first.

I

White speaks as if there were no difference at all in the function of 'true' and 'false' as applied to statements, and as applied to objects; indeed, on his treatment there *is* no difference; in both cases the evaluative meaning is the same and the descriptive meaning is different for different contexts, just as it is for 'good'. But there is in fact a lot of difference between 'true' as applied to statements, which is its primary application, and as applied to objects, which is a secondary one. That there is something odd about the application to objects may be suspected on comparing White's sample list of objects which admit the epithet 'true' (friends, Conservatives, Englishmen, freedom, temperance, steel, likeness) with his list of objects which admit the epithet 'false' (teeth, nose, manner, front): it is surely odd that the lists are *different*. Whether 'true' and 'false' are behaving here as the ordinary polar opposites they are usually taken to be, or as the scalar opposites that White takes them to be, in

either case it would be very odd that scarcely one of the objects listed can take both epithets. ('Friend' is perhaps the only one.) This strongly suggests that their use in such cases is a secondary one, the availability of which requires further conditions to be satisfied than in the case of statements.

The best proof that we have here two uses and not one comes from considering that the epithet 'true' can be applied in two entirely different ways—corresponding to the difference between statements and objects—to the very same entity. White distinguishes between statements and objects, as of course he must, but in a sufficiently liberal sense of 'object' statements are themselves a kind of objects and are named by means of *oratio obliqua* noun-clauses. We can always ask of such an object whether it is a genuine instance of its kind, whether it is a statement or not—just as we can ask of a certain person whether he is a friend or not; and having decided that it is, we could go on to commend it as a "true" statement (and not a question or piece of nonsense), just as we say that someone is a true friend (and not a sycophant). This would have nothing whatever to do with the *truth* of the statement in its ordinary sense, and in practice we do not perpetrate this particular pun.

The relation between these two uses is fairly simple. 'X is a true Y' means, very roughly, 'The statement that X is a Y is true'; more nearly, 'If anyone says or were to say that X is a Y, what he says is or would be true'; more nearly still, 'If anyone says or were to say that X is not a Y, what he says is or would be false.' This shows that the formula 'true Y' is a condensation of a meta-statement in which 'true' has its standard, primary use as applying to a statement. The first hypothetical version brings out the point that in calling someone, say, a true friend, we do not necessarily have any statement in mind. The second hypothetical brings out the point that, if we do, then it is because we first have in mind the possibility of doubting that he is a friend. 'He is a true friend', in short, rebuts 'He is a false friend' which itself rebuts 'He is a friend' while conceding that he may well appear or claim to be one. Instead of 'He is a true friend' we could equally well say 'He is a friend', the emphatic assertion now serving to rebut the implied denial. Of course all this is highly schematic; in ordinary conversation these thrust-and-parry moves may lie far below the surface.

Indeed they must do, if White's next move is to be at all plausible. For he needs to extract an evaluative element from the 'true X' type of example, in order to move back to true statements in the hope of convincing us that there must be the same evaluative element there too.

To say that a person is a 'false friend' is . . . to grade him low. . . .
To say that a mathematical statement is true is to mark it high . . .
(p. 319).

And, generalising, he says that "'true' as well as 'good' praises, directs, guides, encourages, agrees, endorses, expresses a favourable

attitude, sets up standards" (p. 330). In fact it does none of these things, except incidentally and in conjunction with other factors which need to be made explicit: 'good' needs no such qualifications and there is no significant parallel. For consider why it is that 'Smith is a false friend' does express an unfavourable attitude. (I prefer this to White's "grade him low" because this is open to my other main objection.) We do not object to Smith because it is not the case that he is a friend, but because he has allowed or encouraged us to think that he is one, although he is not; he has been guilty of deceiving us. It is masquerading, too, that we object to in another of White's examples, manner; in the case of teeth, however, it is not; rather, I suppose, it is just that we prefer natural teeth to artificial ones. As for his last example—front—I am not sure what he has in mind, but in the case of a false *bottom* it seems clear to me that there is no grading or adverse attitude whatever.

Nor is there any general pro-attitude associated with 'true', except where this can be traced to other sources: it would be tedious to repeat the above procedure with White's list of examples. They are so chosen that in practice a pro-attitude would indeed almost certainly be expressed. But let us choose another example where it would not. Suppose a botanist friend says to me, "What you call a geranium is really a zonal pelargonium; the true geranium is the cranesbill". Next time we are out in the country he points to a cranesbill and says "*That* is a true geranium". Here he is certainly not commending the flower, as the speaker in our other example was no doubt commending the friend. If he is commending anything, it is the taxonomic competence of a would-be or might-have-been speaker saying "That flower is a geranium"; such a speaker is being commended for getting his description right. In the other case, the friend was being commended for matching up to a certain description, for having in fact the properties connoted by 'friend'. 'True' takes all these evaluative differences in its stride.

Not only may 'true X' be evaluatively non-committal towards X: the evaluation may even be reversed. A sample list could be compiled to rival White's: villain, misery, depravity. White of course has not ignored this; he says (p. 320) that a Socialist as well as a Conservative can call a man a 'true Conservative' and both mean the same except that the Socialist is expressing an unfavourable attitude instead of a favourable one. But he insists that "'true' nearly always expresses a favourable attitude", only adding that "of course it can, like 'good' and many other words, become derogatory in certain contexts". But the reason why 'true' more often than not expresses (or rather, according to me, contributes to the expression of) a favourable attitude is totally different from the reason why 'good' does. 'Good' always and invariably expresses a favourable attitude, except where it is used ironically or in what Mr. Hare has called its "inverted-commas" use: that is to say, in speaking of a good X I may not be commending X, I may mean 'the sort of X that

certain people, but not myself, would commend and call good'. This is because the evaluative force of 'good' does not depend in the least on the sort of thing that X is; all that depends on that is the descriptive aspect, the criteria according to which X is evaluated. Thus in order for 'good' to be used derogatorily, it has to be understood that the X in question is the sort of X that is valued by a group of people whose value-judgments are rejected by the speaker's group. A duality of criteria is thus involved. The evaluative force attached to 'true', on the contrary, does depend entirely on the sort of thing that X is or purports to be, and there is no question of dual criteria. If Xs are the sort of things to which we already have a pro-attitude, then the bare statement that this is an X puts us in gear, as it were, for the expression of this attitude; the addition of 'true' lets in the clutch. If we are adversely disposed towards Xs, then we go into reverse; while if we are evaluatively indifferent, the word 'true' is also evaluatively neutral, as in the case of the geranium. As for 'false', this too is a clutch-engaging word, only in this case we change from forward to neutral or reverse at the same time. Our pro-attitude to the object, as being an X, is cancelled by the formula 'false X', or else transformed into its opposite, as described above in the case of 'friend'. Again there should be room for adverse attitudes which 'false X' can switch in the same way, and neutral ones which it leaves unaffected. 'False bottom' would be an example of the latter. There are probably not many actual examples of 'false X' where Xs are the object of an adverse attitude which 'false' reverses, but one that comes to mind is 'false alarm'.

White's argument has thus been demolished in so far as it rests on (1) inferring by analogy that what can be said of 'true' when it qualifies objects can also be said when it qualifies statements: but it is the latter use that is primary and the former analogous or derivative; (2) claiming that what can be said of 'true' when it qualifies objects (and consequently, by (1), also when it qualifies statements) is that it always has an evaluative element: but there is no evaluative element in 'true' as applied to objects, except what accrues from our attitude, pro or con as the case may be, to the objects themselves. These objections to White's argument do not, however, by themselves refute his conclusion, that 'true' as applied to statements does contain an evaluative element.

II

In pressing his defensive attack against the objection, which he attributes to Mr. W. V. Denard, that evaluative words always imply a grading scale, whereas 'true' and 'false' "are not so obviously used in this way" (p. 322), White rather surprisingly abandons his object-statement analogy; surprisingly, because it would have been easier to find colloquial examples of comparative uses among his lists of objects characterizable as 'true' and 'false' than to find

plausible examples involving statements. 'No man ever had a truer friend'; 'This is a truer likeness than that': these have a somewhat truer ring about them than White's own 'truer account or description'. Personally I find 'truer' (not to mention 'falsier') pretty barbarous but have not spent enough time with the *O.E.D.* (or Lewis and Short) to be able to argue *ad hominem* about what we do or do not say. I simply find it impossible to attach any sense to such a sentence as "This statement is more true than that", pending explanations: for instance (a possibility that White notices) this might be said of a compound statement, meaning that it contains more true sub-statements and fewer false ones than another compound statement. But failing any such explanation, it makes no more sense to speak of one statement being more true than another, than it does to speak of one marksman hitting his target more successfully than another marksman hits his, or of the horse that finishes first in its race being more a winner than another horse that finishes first in another race—not that a sense cannot be given even to these, if appropriate special explanations are forthcoming (one bullet was nearer the centre of the target, one horse won by a longer distance, etc.). The very obviousness of these explanations, and of the need for them, shows that the absence of grammatical comparative and superlative forms of the adjectives 'true' and 'false' is no philological accident.

White betrays a certain uneasiness about this for he hints at withdrawal to a second line of defence: if we insist on denying a comparative use of 'true', we could, he says, "parallel this with other evaluative words, like 'right', which have no degrees of comparison". Those who, like myself, endorse Mr. Denard's objection are not going to be much impressed by this; the obvious reply is that, if 'right' is indeed a non-comparative word, which it certainly is, then 'right' is not an evaluative or 'grading' word either. But there is good reason for White's uneasiness here. It is that both 'true' and 'right' are often at work in contexts which involve both polar and scalar appraisals. (I continue for the present, for the sake of argument, to use White's term 'appraisal' non-committally.) In their own right 'true' and 'right' are purely polar terms: a statement is either true or false, an action is either right or wrong. But just as we can find examples which seem to lend support to a comparative use of 'true' and 'false', so we can for 'right' and 'wrong'. Just as one statement is not quite true and another is very far from the truth, one account is slightly inaccurate and another is a pack of lies: so one way of tying a knot may be not quite right and another hopelessly wrong, or of two criminal offences against the same law, one may be much graver than the other. This contrast between polar and scalar appraisals is particularly well illustrated in judicial procedure, since in the British system at least they are assigned to different functionaries. The jury decide whether the prisoner is guilty or not guilty: this is a polar appraisal, in fact

the paradigm of polar appraisals, namely a *verdict*. The judge decides what sentence to pass in the light of his assessment of the gravity of the offence; this is a scalar appraisal. But since both operate in the same field, it is understandable that they get conflated and we are tempted to speak of one action being 'more wrong' than another.

In the field of statement-making discourse, it is not hard to find cases of dual operation almost as clear as the judicial one. Take White's example, already quoted, of a mathematical statement, of which White says that to call it true "is to mark it high". If the mathematical statement is the answer to a question in a mathematical examination, anyone who has marked such papers (or papers in formal logic, for that matter) may well ask, "Why not full marks?" Mathematical statements are just the sort of statements for which Yes/No, non-scalar adjudications are appropriate—indeed 'right' and 'wrong' are much more at home here than 'true' and 'false'. Of course in practice marks will be deducted for uneconomical proofs, bad handwriting and so on, and when this happens a scalar system of assessment has been superimposed on a polar one. Examiners in these subjects are familiar with the difficulties of working with the two systems together.

The reason why 'true' and 'false' sometimes operate in a seemingly comparative way is not typically, then, as White suggests on behalf of his imaginary opponent, a matter of aggregates containing more or fewer parts that are simply true or false. Even a simple, non-compound statement can be 'further from the truth' than another. Now there is only one (polar) criterion of truth; or at most (I am not taking issue on this) there are as many criteria of truth as there are *types* of discourse (empirical, mathematical, scientific, etc.). But there are as many (scalar) criteria of "degrees of approach to/departure from the truth" as there are *subjects* of discourse. For example, consider the statements "That is a tractor" and "That is an Austin-Healey". If the thing in question is a vintage Bentley, both the statements are false, but one is "further from the truth" than the other is. In this case the scalar difference turns on the scale of degrees of resemblances among motor vehicles, but with another subject another scale would come into play.

'True' and 'false' then are polar opposites. When a statement has been appraised as false, then another, scalar, criterion may be put to work, yielding a new range of appraisals expressible in comparative terms like "nearer to/further from the truth". This does not apply to statements appraised as true, and in particular the appraisal 'true' is not a specially high or the highest rating on this scale: for there is no such point on this scale, any more than knowledge is an extra high point on the scale of degrees of belief.

Clearly these points cannot be brought out so long as the battery of terms 'appraise', 'assess', 'evaluate', 'grade', 'rate', 'rank' and so on is used indiscriminately, for example of 'right' as well as 'good'. Much of course has been written about the relations

between 'right' and 'good', but not enough, evidently, to prevent philosophers from going on using blanket terms like 'evaluative' to cover them both. We need a new set of terms, of which 'verdict' will be the paradigm member, to distinguish polar from scalar expressions; our vocabulary is perhaps not so rich in them as it is in near-synonyms of 'grading'; but besides 'verdict' we have 'decide', 'pass/fail', 'allow/disallow', 'find for/against', and if these are not enough we must make do with the polar words themselves and do without, or else invent, names for the functions they perform. White is not alone in failing to distinguish between the two sets; Professor Nowell-Smith, for instance, explicitly assimilates them: he has a section in his *Ethics* (12.2.(c)) headed "Verdicts and Appraisals" in which he treats them quite indifferently, yet his remarks there are all about 'good' and 'good' is not, on my view, a verdict-word at all, so that what he says of appraisals is not necessarily true of verdicts.

Of course it can be argued that we still need a general term expressing what both evaluations and verdicts have in common. But such a term ought to be explicitly introduced and not covertly used to conceal important differences. I do not agree either that 'appraisal' is a suitable term, or that there is any other, or that we need one. It seems to me that the list of performances: commending, applauding, encouraging, signalling a bull, reporting a jury's decision, marking with a tick or cross, and so on, is far too heterogeneous to be worth collecting under a single concept, or even under my twin concepts of polar and scalar—that distinction itself should only be a pointer to others. And the list of performances we undertake with 'true', corroborating, emphasising, endorsing, correcting, allowing, conceding, and so on, is itself almost as heterogeneous. ('Pro-attitude', for example, fails to cover the last two.) In this note I have only tried to suggest certain divides and to re-site some of the more straightforward uses of 'true' on the right sides of them.

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AMBIGUITY AND INTUITION

THE point of this paper may be put in a few words—it is simply that while we are able intuitively in many cases to detect the occurrence of ambiguity, it is extremely difficult, if not impossible, to specify a usable criterion for the determination of such occurrences. Since the demonstration of lack of cogency in philosophical arguments frequently turns on the demonstration of the ambiguity of terms employed in these arguments (equivocation), and since showing that a philosophical "dispute" is "merely verbal" involves similar considerations, this point is of some philosophical importance. It will be obvious that consideration of a criterion of ambiguity is of particular importance in those cases in which there is disagreement as to whether or not terms employed in philosophic discourse are ambiguous. I am by no means convinced that any more fundamental appeal than one to our intuitive (direct) understanding of the meaning of terms is possible, although, of course, the judicious use of examples may be an invaluable help to such understanding.

It may be well first to note an ambiguity in the term 'ambiguous'. The term is generally used simply to refer to an expression having more than one meaning. We may call this *semantical ambiguity*. But sometimes the term 'ambiguous' is used of an expression only when the expression—which is semantically ambiguous—occurs in a context in such a way as to be misleading, or in such a way that the intended interpretation is not clear.¹ Let us call this *psychological ambiguity*. Note that psychological ambiguity involves semantical ambiguity, but not conversely. Although most interesting cases of ambiguity involve psychological ambiguity, I shall be concerned with the more general case, that is, with semantical ambiguity.

A suitable point of departure for our discussion is provided by a quotation from John Hospers.

As a glance through the dictionary will show the majority of the words in the English language are ambiguous in one way or another. Yet we should be careful not to "detect" ambiguities when they are not there:

(1. A word is not ambiguous simply because it stands for many particular things. The word "see" is not ambiguous because you can see a horse and then see a tree. Both are instances of seeing, and a word is not ambiguous just because there are different instances of its application. The word "see" means the same in both cases, though of course the things seen are different. The word "see" can be used ambiguously: when we say "Do you see what I'm driving at?" we make no reference to vision. It is this last use and not the

¹ This use was pointed out to me by my colleague, Professor Bertram Jessup. Cf. John Hospers, *An Introduction to Philosophical Analysis* (New York: Prentice-Hall, 1953), p. 23, "Sometimes, in fact, the very word 'ambiguity' is restricted so as to mean only *misleading ambiguity*. . . ."

existence of many *instances* of seeing (in the visual sense) that makes the word ambiguous.

2. A word is not ambiguous because we can break up the class of things it refers to into smaller classes or sub-classes. We can break up the class of colours into the class of reds, the class of yellows, etc. But the word "colour" is not made ambiguous by the fact that red, yellow, etc., are colours. They are colours in exactly the same sense of the word "colour". (A different sense of the word is being used, however, when we say "Her personality has so much colour.") The fact that there are kinds of X does not make the word "X" ambiguous; it is ambiguous when it is used in two or more senses, that is, when it is used to stand for two or more classes of things.¹

With one proviso, I find myself in agreement with what Hospers is saying. The proviso has to do with the last sentence, in which he appears to equate a term's being used in two or more senses with its referring to two or more classes. It would appear rather that denoting two or more classes is a sufficient but not a necessary condition of a term's being ambiguous, *i.e.* of its being used in two or more senses. This is simply a corollary of the semantical principle that sameness of intension implies sameness of extension, but not conversely. ('Green centaur' is an ambiguous term since it may be used to mean centaurs of a certain colour, or centaurs of a certain degree of experience; the classes referred to, however, are both identical, both being empty).

I have indicated that I agree with Hospers (a) that "a word is not ambiguous just because there are different instances of its application", (b) that "a word is not ambiguous because we can break up the class of things it refers to into smaller classes or sub-classes", and (c) that a term is ambiguous "when it is used to stand for two or more classes of things". Let us call these three conditions (a-c) *ambiguity-criteria*. I have also indicated that I feel that an appeal to a sense of language, or to an intuitive understanding of the way terms are used, is essential for the detection of ambiguity. (I believe that such an appeal is also required in recognizing synonymy, analyticity, etc.—the familiar Quine-White syndrome—but our present problem is that of ambiguity.)

It might appear that the three points from Hospers provide us with an extensional criterion of the presence or absence of ambiguity, but I shall attempt to show that this appearance is misleading. It is misleading, first, because the determination of what class of individuals—or, phrased alternatively, what individuals—is (are) denoted by a given term, is dependent on the understanding of the meaning or intension of that term, since classes are determined (*i.e.* here, delimited or fixed) by certain properties. But, what is more, even if we could somehow determine what a given term denotes apart from some direct understanding of the terms, we still could not determine whether or not the term is ambiguous without such intuitive understanding. Stated abstractly in terms of our ambiguity-

¹*Cf.* John Hospers, *An Introduction to Philosophical Analysis* (New York: Prentice-Hall, 1953), pp. 24 f.

criteria, simply knowing the extension of a term will not enable us to distinguish between the satisfaction of condition (b) a condition of non-ambiguity, and of condition (c), a criterion of ambiguity. Thus, if we consider everything denoted by a given term, we do not have a sufficient basis for determining whether we are confronted with a single class which is capable of being divided into sub-classes—condition (b)—or with two or more classes—condition (c). Indeed, assuming that the logical sum of two classes is itself a class, it is obvious that the “problem” is unsolvable. (“It all depends on how you look at it.”) And yet, we do decide with a remarkable degree of unanimity that some terms are ambiguous and that others are not!

It will be useful to develop this point concretely by using examples, especially since what is needed for a grasp of some of the difficulties involved is simply some practice in looking at familiar terms in slightly unfamiliar ways. We shall proceed from a simple case to more difficult ones.

First, let us consider the case of a term which is obviously ambiguous; one, indeed, which Hospers takes as a paradigm of ambiguity—the word ‘see’. Among the meanings of this word are, stated roughly, perceiving with the eyes, and understanding; as in “I see the desk”, and “I see the point of the argument”, respectively. That ‘see’ is ambiguous is obvious from these examples. We say that ‘see’ is used literally in the former example, and figuratively in the latter, that ‘seeing’ in the second sense represents a metaphorical extension of the original (literal) meaning. (It is obvious that the literal-figurative distinction suffers *inter alia* from whatever difficulties attach to the ambiguous-unambiguous distinction.) Let us suppose, however, that someone were to deny the ambiguity of ‘see’. Suppose, that is, that this individual were to maintain that the term ‘see’ is used in the same sense in each of the foregoing examples. We may assume that he is aware of the facts involved, that in one case we see with our eyes, and in the other in some other way (“with our mind”). He might nonetheless contend that we are simply confronted with two sub-classes of seeing, *viz.* the optical and the non-optical. He might point to the fact that ‘perceive’ is not rendered ambiguous by the circumstances that we perceive sometimes with our eyes and sometimes with our ears, etc. That is, that there are optical and non-optical perceptions does not show an ambiguity in the term ‘perceive’. To perceive a sound and to see a fallacy are both non-optical processes, and they are so because of the subject matter being dealt with in each case.

Much more might be said in defense of the view that ‘see’ is unambiguous. I am not concerned to defend the view, and I feel that I have said enough to indicate the general grounds on which it might be defended. I trust, also, that I have said enough to render plausible to the reader the point that it is difficult to imagine a workable criterion of ambiguity which is totally independent of an appeal to direct understanding of meanings.

I should like to point out that I am not defending such appeal to direct understanding as desirable. Indeed, I find it distinctly undesirable, since it is likely to leave disagreements unresolved. But it is clear that the way we are taught to use terms will be decisive for the way we understand and use them. And it is obvious that this teaching will vary from individual to individual.

By considering examples, we could proceed indefinitely in our discussion of the difficulties with the application of the term 'ambiguous'. Let us, however, very briefly consider the bearing of our discussion on just two philosophical terms, one, the semantical 'meaning', and the other, the ontological 'exists'.

The allegation is frequently made that failure to distinguish two senses of the ambiguous term 'means' is a significant source of philosophical error. When the question arises as to what a certain term means, we are told that we must distinguish between the question of what the term denotes or refers to, and the question of what the term connotes. It is said that the term 'meaning' is ambiguous as between extension and intension. By subsuming all cases of meaning under the rubric of denoting, it is maintained, we are led to postulate or to believe in a host of dubious entities of an abstract sort: properties, relations, prepositions, etc, without attempting to discuss even what is at stake in a consideration of the ontological status of meanings, let us indicate what a philosopher might say who denied the ambiguity of 'meaning'. First, he would presumably grant that when we ask what some term means, we must distinguish the question of what particulars the term denotes from the question of what properties it connotes. But he would add that this fact is insufficient to demonstrate that 'means' is ambiguous, since the relation between term and extra-linguistic elements is the same in both cases. Only the *relata* are different.¹ That is, denoting and connoting are two sub-classes of the generic concept of meaning or designating. Such a person would be emphasizing similarities between the two concepts, while his opponent would be emphasizing dissimilarities.² He might go on to add that it is his opponent who is misled by the latter's insistence on the ambiguity of 'meaning' into thinking that there are no properties, relations, etc. But we approach the forbidden ground.

Let us turn away to the question of the ambiguity of 'exists'. Professor W. V. Quine, in a series of well-known articles,³ has attempted to set forth a criterion of ontological commitment. He maintains

¹ Cf. Gustav Bergmann, *Philosophy of Science* (Madison, University of Wisconsin Press, 1947), p. 13. "Surely there is a difference between character words and proper names, but there is no difference in the way they name."

² Cf. John Wisdom, "Philosophical Perplexity", reprinted in *Philosophy and Psychoanalysis* (Oxford: Basil Blackwell, 1953).

³ See, e.g. "On What There Is", *Review of Metaphysics*, ii (1948), 189-206; reprinted in Leonard Linsky (ed.), *Semantics and the Philosophy of Language* (Urbana, The University of Illinois Press, 1952).

that we are committed to the existence of those entities over which the variables of our quantified assertions range. Thus, for example, we are committed to the existence of abstract entities when we assert that some species are cross-fertile or that there is a prime number between 1,000 and 1,010.¹ Some of Quine's opponents have maintained that his views on ontological commitment are defective because, in effect, he has failed to recognize the ambiguity of 'exists': he has failed to realize that we are saying a different *sort* of thing when we assert that there is a prime number in a certain range than when we assert that there is a prime steer on a certain range. Thus, they argue, the general ontological problem cannot be solved by consideration of what is said—in true statements—to exist. Quine's reply to this allegation is instructive for our present purpose.

We have all been prone to say [he writes], in our common-sense usage of 'exist' that Pegasus does not exist, meaning simply that there is no such entity at all. If Pegasus existed he would indeed be in space and time, but only because the word 'Pegasus' has spatio-temporal connotations, and not because 'exists' has spatio-temporal connotations. If spatio-temporal reference is lacking when we affirm the existence of the cube root of 27, this is simply because a cube root is not a spatio-temporal kind of thing, and *not because we are ambiguous in our use of 'exist'*.²

Now, of course, Quine's view can be maintained. We can maintain that 'exists' is unambiguous, and that there are two sub-sets of existents, the spatio-temporal and the non-spatio-temporal. But it obviously can just as well be maintained that we are confronted with an ambiguous term, that there are two distinct kinds of existents, two distinct senses of 'exists'. Apart from a direct understanding of the term 'exist', apart from a grasping of the essence of existence—to use particularly un-Quinean language to describe the operation which Quine has apparently undertaken in the quoted paragraph—no resolution of the disagreement appears possible.

I might have mentioned other examples. Thus, is 'true' ambiguous as between logical and factual truth? Is there a single concept of probability,³ or are there two⁴ (or more), as Carnap has maintained? But I trust that the point is clear.

To sum up: there are some terms which are pretty clearly ambiguous; if there weren't, the concept wouldn't be as useful as it is. But if a question arises as to whether or not a term is ambiguous ("is *really* ambiguous" may suggest something of the difficulty), then it is not answerable in any simple way. To see this, consider a disputed general term T. The dispute over T's ambiguity arises presumably because what T denotes can be divided into two (or more)

¹ *Ibid.* p. 200.

² *Ibid.* p. 191. Italics not in original.

³ See, C. I. Lewis, *An Analysis of Knowledge and Valuation* (La Salle, Illinois, the Open Court Publishing Co.), ch. x, and esp. p. 314.

⁴ See, *Logical Foundations of Probability* (Chicago, The University of Chicago Press, 1950), Sec. II, esp. pp. 23 ff.

classes, α and β , with defining characteristics ϕ and ψ respectively. The question of the ambiguity of T turns on the question of whether or not α and β are sub-classes of a larger class, or of whether ϕ and ψ have some characteristic in common. The obvious answer that α and β are sub-sets of $\alpha \vee \beta$, or that ϕ and ψ share the characteristic ' $\phi \vee \psi$ ' is not intuitively satisfactory. The question is this, are ϕ and ψ *sufficiently alike* (in some unspecifiable way)? If they are, T is unambiguous; if not, not.

My rather unsatisfying conclusion is the following: if recognition of ambiguity rests on direct understanding of the meaning of the term in question, and if an appeal to such understanding is unsatisfactory in adjudicating disputes as to the presence or absence of ambiguity, then it would seem that there is no satisfactory method of settling such disputes; unfortunately, both components of the antecedent of this conditional appear to be true.

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LOGICAL TRUTH AND THE LAW OF EXCLUDED MIDDLE

IN a recent note¹ N. E. Christensen has charged that there is a vicious circle in Quine's definition of logical truth. This definition is (roughly) that a statement is logically true if it and all statements of similar form are materially true. Mr. Christensen's argument has two parts. (1) In order to verify that the statement "Either it is raining or it is not raining" (hereafter "Either R or not R") is materially true, one must first ascertain by observation that it is raining (i.e. that R is true) and then must "discover" that this is sufficient to make the disjunction in question true by "further applying truth-value analysis".² (2) However, in applying truth-value analysis we presuppose the law of excluded middle—that every statement is either true or false. Mr. Christensen then asserts that this formulation of the law of excluded middle "tells us the very same"³ as the assertion: "the statement form ' $p \vee \sim p$ ' is tautologous." He quotes a passage from Quine's *Mathematical Logic* (p. 51) which indicates that Quine would accept this identification. Thus in order to establish the material truth of "Either R or not R" one must appeal to the principle that " $p \vee \sim p$ " is tautologous". On the other hand, one establishes that "Either R or not R" is logically true because it is one instance of the tautologous statement form ' $p \vee \sim p$ '. It follows from this that any attempt to define what one means by the logical truth of "Either R or not R" in terms of the material truth of "Either R or not R" (and other statements of similar logical form) necessarily involves one in a vicious circle.

It seems to me that both parts of Mr. Christensen's argument are faulty. With regard to the first part, Christensen's idea that we need to use truth-value analysis in order to "discover" that "Either R or not R" is true, given that R is true, is very odd indeed. The only sense in which one would have to refer to truth function theory at all would be to look up the definition of "or" (\vee); if we did this, as Mr. Christensen himself notes, we would "easily discover" that "Either R or not R" is true, given that R is true. But surely one does not have to understand truth function theory in order to know the meaning of "or". I dare say that there are many people who are incapable of grasping even the elementary definitions of truth function theory, but who nevertheless can see that "John is either at the zoo or the movies" must be true, if John is at the movies. It seems to me, therefore, that the first part of Christensen's argument is mistaken.

I think that the second part of Christensen's argument is mistaken

¹ MIND, July 1957, p. 395.

² *Ibid.* p. 396.

³ *Ibid.*

also, though the points at issue here are more knotty. First, suppose that we do use the truth-functional definition in order to verify "Either R or not R" on the basis of R's truth. In what sense does this "presuppose" the law of excluded middle? It is true that there is a certain sense in which the ordinary truth-functional definition of "or" conforms to the law of excluded middle, since under this definition "Either—or—" statements will always be either true or false. But, if one were to use a definition of "or" which did not conform to the law of excluded middle (and thus would, in this sense "presuppose" the law to be false) would this prevent one from verifying "Either R or not R" given that R is true? A definite "No" answer to this question is furnished by the definition of "or" in the classical three-valued system of Lukasiewicz. This may be seen by inspecting his definition.

R or S	T	I	F	Here T = true
T	T	T	T	I = indeterminate
I	T	I	I	F = false
F	T	I	F	

The first row here shows that whenever R is true the disjunction will be true. It may be objected by those with strong feelings of attachment to two-valued logic that this definition is of purely formal interest and has no connection with the ordinary "or". But this definition does conform to what is most clear in our ordinary concept "or", namely: "R or S" is true if and only if at least one of R or S is true; and I do not think one can say much more for the usual two-valued definition. For these reasons, I think that the verification of ordinary "Either—or—" statements does not necessarily presuppose the law of excluded middle.

Another step in the second part of Christensen's argument which I am inclined to question is the identification of the law of excluded middle with the theorem " $p \vee \sim p$ is tautologous" (hereafter abbreviated as T). What I have in mind may be made clear by a brief consideration of truth function theory. In setting up this theory two rules are used from the very beginning. (a) The Rule of Contradiction: In any space in a truth table one cannot have both a "True" (T) and a "False" (F). (b) The Rule of Excluded Middle: In any space in a truth table one must have either a "T" or an "F". Using these rules, which are usually simply taken for granted and not even stated, one can easily show by the method of truth tables and by the definitions of " \vee ", " \sim ", and "tautology" that T.

The functions of the Rule of Excluded Middle and T are quite different within truth function theory. It seems correct to me to say that any use of truth-value analysis, including reference to truth table definitions of statement connectives, presupposes the Rule of Excluded Middle. This cannot be said of T which, as has been pointed out frequently, is only one theorem among others of truth function theory. Now, Mr. Christensen is correct in asserting

that in order to establish the logical truth of "Either R or not R" we would appeal to the fact that it is an instance of " $p \vee \sim p$ " which T asserts to be tautologous. But in order to establish the material truth of "Either R or not R" one would not need to appeal to this fact even if one were to use truth-value analysis; rather, what we would need to use or presuppose would be the Rule of Excluded Middle. Since "Either R or not R" obviously is not an instance of the Rule of Excluded Middle, Mr. Christensen's circle would be broken.

Most modern logicians have followed *Principia Mathematica* in referring to T as the law of excluded middle, just as Quine and Christensen do. Despite this, it seems to me that if anything within truth function theory deserves to be called the law of excluded middle, this title should be bestowed upon what I have called the Rule of Excluded Middle. This Rule, together with the Rule of Contradiction, is absolutely fundamental to the whole method of truth-value analysis. The two rules thus have within truth function theory something like the importance assigned to the laws of contradiction and excluded middle by traditional logicians. On the other hand, the theorem T is just one theorem among a large number of equally important ones in truth function theory. Indeed, within the theory it can be shown by truth-value analysis to be logically equivalent to such theorems as " $p \supset . q \supset p$ " for example, which no one would be tempted to associate with the law of excluded middle.

One consequence of regarding T as the law of excluded middle is the following. When this is done, the theorem C: " $\sim(p \sim p)$ " is tautologous" will be identified with the law of contradiction. But within truth function theory C and T are logically equivalent. The following short demonstration shows this.

$$\begin{aligned} p \vee \sim p &\equiv \sim(\sim p \sim p) && \text{De Morgan's Law} \\ &\equiv \sim(\sim p . p) && \text{Law of Double Negation} \\ &\equiv \sim(p \sim p) && \text{Commutative Law for Conjunction.} \end{aligned}$$

On this basis, it is sometimes asserted that the laws of contradiction and excluded middle are logically equivalent and that one cannot assert one and deny the other without contradiction. From this the conclusion is drawn that any genuine "alternative logic" such as Lukasiewicz's three-valued system is impossible. One example of an argument against "alternative logics" along somewhat these lines is contained in Herbert Feigl's well known paper "De Principiis non Disputandum . . . ?"

If, however, the law of excluded middle is identified with the Rule of Excluded Middle, genuine "alternative logics" are possible. It is obvious, I think, that the Rules of Excluded Middle and Contradiction are independent of each other. In Lukasiewicz's three-valued system the Rule of Contradiction continues to hold, though

¹ In *Philosophical Analysis*, ed. Max Black (Cornell 1950). Cf. especially pp. 125-128.

it is now just a special case of the wider rule that in any space of a truth table one can have at most one "T" or one "F" or one "I". The Rule of Excluded Middle is definitely dropped however; it does not follow from the corresponding three-valued rule that in each space there must be at least one "T", "F" or "I".

If "or" is defined in a three-valued system in the way given above, and one defines negation as Łukasiewicz does, then the statement form " $p \vee \sim p$ " ceases to be a tautology.¹ The following is the truth table illustrating this.

p	$\sim p$	$p \vee \sim p$
T	F	T
I	I	I
F	T	T

Some might say that this only shows that the ordinary meaning of "or" has been changed. I have indicated one answer to this objection above. It might also be objected that statements like "Either R or not R" are obvious logical truths and, therefore, that " $p \vee \sim p$ " must be tautologous, if the usual connection of the concepts of logical truth and tautologous statement form is to be maintained. If this is so, no genuine denial of the law of excluded middle would be possible and three-valued systems with their denial of the Rule of Excluded Middle would be of purely formal interest. A consequence of this is that my suggestion that the law of excluded middle be identified with the Rule of Excluded Middle would be mistaken.

My reply to this objection is to assert that there is some intuitive reason to hold that "Either R or not R" is not a logical truth. It seems to me that there are circumstances in which one might hesitate to affirm the truth of this statement. I suppose that everyone has had occasion to walk on certain misty days when the moisture in the air is visible as a light mist and one's face and clothes gradually become wet from moisture deposited on them as one walks. At the same time, one does not see drops of moisture falling towards the earth, nor can one actually see the moisture being deposited on one's clothes or skin. Under such circumstances, it would not seem correct to describe the weather by saying either "It is raining" or "It is not raining". And I believe that under these circumstances most people would hesitate to answer simply either "Yes" or "No" to the question "Is it raining?" The concept "rain" has an area of vagueness where one hesitates to say whether it or some other concept such as "mist" applies to the weather. Now, if the "I" in the three-valued truth-tables is interpreted to indicate this situation where one would hesitate to say whether the sentence "It is raining" is a true or a false description of the weather, the statement "either R or not R" could be taken as an instance of " $p \vee \sim p$ " as this is (non-tautologously) construed in the three-

¹ This fact, that when the Rule of Excluded Middle is dropped " $p \vee \sim p$ " ceases to be a tautology, is possibly one reason why there is a tendency to identify T with the law of excluded middle.

used truth table above. The second row of the table represents the situation I have been describing. Mr. Geach has recently noted that all empirical concepts are vague in the same manner that "rain" is; he has suggested that an exact logic of such vague concepts should be developed.¹ The trend of the above remarks is to the effect that three-valued logic might be construed as this exact logic. A great deal more would have to be said in order to establish this thesis however.

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¹ Cf. "Dreams and Self-Knowledge", *Aristotelian Society, Supplementary* vol. xxx, 71-73.

MR. HAMPSHIRE AND PROFESSOR HART ON
INTENTION: A NOTE

IN their article, (MIND, January 1958), Mr. Hampshire and Professor Hart are, at one point, concerned to reject as inadequate a particular analysis of the notion of intentional action. I want to question the force of the authors' stricture on this analysis. More especially I wish to maintain that the analysis in question is substantively correct and is only incomplete in the form offered.

According to the authors, the particular view they are disputing states that:

The primary point of saying that someone acted intentionally is to rebut a *prima facie* suggestion that he was in some way ignorant of, or mistaken about, some element involved in this action. Usually, the suggestion arises from the fact that what he has done is abnormal or wrong in some way or that it is something which ordinary people would not do except unintentionally. Part of the force of 'He did it intentionally' is just to rule out the suggestion that he did it unintentionally, where 'unintentionally' means that the agent did what he did through accident or by mistake. This suggests that the whole meaning of 'intentionally' simply lies in its negation of accident or mistake, and that once these two ideas are elucidated, as they easily can be, the analysis of the notion of intentionally doing something is within our grasp. On this view the analysis will simply be (1) a description of the appropriate context for the use of the expression 'intentionally' as rebutting accident or mistake, and (2) the elucidation of the ideas of accident and mistake (p. 7).

Before going on to the authors' objections, we should notice a vagueness in the foregoing account as to just which 'abnormal(ities) and wrong(nesses)' "He did it intentionally" rebuts and so which 'He did it unintentionally' concedes. 'Ignorance' and 'mistake' are mentioned first, and then the general categories of "abnormal(ity) or wrong(ness) in some way" are mentioned. (The next mentioned criterion, viz. "something which ordinary people would not do except unintentionally" seems to me to be tautological.) Finally, the authors seem to settle upon 'accident' and 'mistake' although they give no reasons for choosing just these criteria (and incidentally dropping 'ignorance'), nor do they show why just these criteria cover all varieties of 'abnormality or wrongness'. It is not clear, moreover, whether 'intentional action' as used by the authors does or does not cover categories such as 'voluntary action', or 'deliberate action'. Most of what they say makes one think that they are giving a very broad sense to 'intentional action' but the 'abnormalities' which they finally choose in their account of the view they are attacking leads one to think that they are at least sometimes thinking of 'intentional action' in its narrower less philosophical sense, and indeed in perhaps a technical legal sense.

The authors now go on to argue the inadequacy of the foregoing

view of (broadly speaking), "intentional action" as they see it, on the following grounds:

. . . the assertion that someone has done something intentionally . . . is not merely the equivalent of the assertion that he did not do it unintentionally, or that he did not do it accidentally or by mistake. Accident and mistake are certainly incompatible with the agent doing what he did intentionally; but the assertion that someone did what he did intentionally does not merely exclude these cases of unintentional action. This may be seen from the following example. A man fires and shoots another: if asked what he was doing, and if he was prepared to give an honest answer, he would identify his action as shooting at someone. But in a perfectly ordinary sense of 'know' he would know that shooting at someone involved making a loud noise . . . in such a case it is clear that he did not make the noise by mistake or accident; it is therefore clear that he did not make it unintentionally. But on these facts it would be misleading to say he intentionally made the noise, for this would suggest that this is what the agent would say that he was doing if asked. Always the expression 'He intended to do it' means more than, though it also includes, 'He did not do it unintentionally' (p. 7).

Now the upshot of all this seems to be that the authors take the analysis under discussion as at most giving necessary but not sufficient conditions for the ascription of intention to an action. The counter-example they offer tries to show that to ascribe intention is to do more than merely to eliminate the possible ways in which the action might be said to be unintentional (in a broad sense). But the offered example is so sketchy, that it could easily be filled in so as to make it quite plausible that we should say that the noise was made accidentally. Thus there are devices which nearly eliminate the sound of gun shots, so 'He intended to make the noise' might be rebuttable by 'It was an accident, the muffler slipped'. Moreover, it seems to me that whatever is substantive in the view being attacked may be retained simply by extending the kinds of 'abnormalities' to which actions are subject, and so the 'abnormalities' which 'He intended to do it' controverts. Of course the kinds of possible ways in which an action might be said (in the broad sense of the authors) to be unintentional are numerous. Professor Austin, who has made the important survey of this field, lists among others, and besides accident and mistake, inadvertence, carelessness, recklessness, and as exemplified in a possible accusation by the gunman's wife, "Did you intend to make that noise and wake baby?", thoughtlessness.

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VIII.—CRITICAL NOTICE

On Selfhood and Godhood. By C. A. CAMPBELL. London : George Allen & Unwin, 1957. Pp. xxxvi + 436. 35s.

IN these two series of Gifford Lectures (1953-55), Professor Campbell has set down some of the fruits of many years' reflection on the problems of natural theology. The first course attempts to validate the notion of the self as an "identical perduring spiritual being" as a basis for theological talk about the soul, the second to validate religious experience conceived in terms of Otto's "numinous consciousness". The whole work is constructive rather than polemical in intention; but since Campbell writes, as always, from the viewpoint of a critical admirer of Bradley, each half of the book has its incidental opponents; it goes without saying that he regards "those twin gods of so much recent British philosophy, linguisticism and empiricism" as unsatisfactory objects of worship, and their devotees occasionally come under explicit fire in the first course; in the second the main target is something called "orthodox or official theism", which seems to mean Calvinism as often as not; but of that more later.

There is a good deal of sound sense in the first course and I feel sympathetic to many of the criticisms aimed at contemporary trends. One does not have to be an idealist to appreciate Campbell's passages on the judgment theory of cognition and the importance of self-consciousness; the strictures on current ethical theories seem to me largely well-founded, especially the three canons of criticism on pages 182-183, which draw attention to certain limitations of the methods of linguistic analysis in discovering the meaning of the moral 'ought'; and I have always had a soft spot for Campbell's theory of moral choice and moral effort, though there is a great deal that it leaves unsaid, even as an introspective account. But apart from a discussion (in Appendix B) of Nowell-Smith's criticisms of his earlier and substantially similar writing on free-will—a discussion from which I think Campbell emerges well—he quite deliberately avoids "sporadic skirmishes on the frontiers" with a philosophical viewpoint so different from his own, and makes few concessions in point of topic and none in point of method to current ways of thought. "Modernists" who are willing to stand aside for a while from their own pre-occupations and take up Campbell's challenge to re-examine first principles might perform a useful task if only by stating in clear English why they disagree with Campbell's contentions; but generally speaking philosophical advance comes from a working out of the conflicts and inadequacies of a given system along the lines laid down by its own internal dialectic of development, and Campbell is making great demands on his public.

However, that in itself is legitimate enough, and the more fully

legitimate the greater the positive contribution made by a book. Unhappily there is more cause for concern over the second and crucial half of the book than over any tactical or other deficiencies of the first. My disagreement starts with Campbell's definition of religion as a mental state, rather than as the total ontological relationship between man and the Supreme Being; and from that point on his validation of religion naturally takes, *via* Otto and Bradley, a Kantian form. For there are two alternative types of explanation of such a phrase as 'the mind is under a necessity to think in such a way'. One is, that man's understanding is a faculty whose function it is to apprehend an object with which it achieves thereby a sort of formal identification—the mind in a sense becomes what it knows, under obedience to the being of that which is presented to it. If on this interpretation a truth is forced upon the mind, the necessity is truly objective, and the most general truths about the nature of things become on this view the sort of truths traditionally called 'metaphysical'. But on the typically idealist interpretation, the necessity is objective only in the sense that thinking-in-such-a-way is a condition of our thinking things as objects, with the notorious corollary that we have no guarantee that the world really is as we must think it. To establish the truths of metaphysics in *this* way is to demonstrate that metaphysics in the old sense is impossible. Now Campbell speaks of doing "metaphysics", and indeed attempts to arrive at the properly metaphysical conclusion that reality is ultimately one and eternal, and so on. But he does so by arguing from the Kantian premise that the mind will not tolerate a certain type of thought—namely, that of differentials united in absence of a ground of union. It is difficult to be sure how far he subscribes to the Kantian sense of rational necessity here; the way in which he argues (Lecture X) that the fact that moral consciousness involves cognition as well as feeling guarantees the reality of the moral order certainly has a realist ring about it; on the other hand he rejects Otto's claim that the numinous consciousness is qualitatively unique and underivable and therefore, like the moral experience, guarantees the objectivity of its deliverances. In the case of religion it is his use of Bradley's theory of contradiction that is crucial.

Bradley seems to assert that reality is self-contradictory on the basis of a stipulated definition of contradiction which does not in fact involve intellectual repugnance but only intellectual dissatisfaction—the mind cannot *rest* in differentials united as such. But this seems not to show that reality as we conceive it is contradictory, or, which comes to the same thing, that the human mind is incompetent to conceive reality as it is if by 'reality' we mean the world of experience. What the argument really shows is that finite reality is not self-explanatory. The differences apprehended in experience are rationally unsatisfactory because we cannot in the last resort see *why* S is P—we have to accept it as brute fact; but this is only a more modern way of saying that we have to accept it as contingent; and

the inference from this, if any, is that the ground of the existence of the finite world lies outside it, and, to save a regress, in a being not itself limited by the possession of any diversity save such as is compatible with a perfect unity. But this line of theological argument—to which Campbell's discussion seems to tend—scarcely differs from the traditional Thomist reasoning. In Campbell's hands, however, the result is a surely unintentional pantheism. On Bradley's principles he concludes that reality as we conceive it is contradictory, and that since this is an intolerable position, reality must be a supra-rational unity. But *what* reality? Campbell talks of "ultimate metaphysical reality" as though it were God, but all that his premises entitle him to mean by it is the world of experience in *its* ultimate reality. In short, Campbell reads our inability to discern a ground of diversity in unity in the world of experience as a *failure* to see that world as it really is, whereas in fact it is precisely the *achievement* of that insight. The world of our experience does *not* contain the ground of its own being, and that is the starting point of natural theology.

Mention of the Thomist tradition recalls the fact that the name and theories of Aquinas—or even of his scholastic opponents—are virtually without mention in Campbell's book. This seems to me a pity. Campbell speaks of his own embarrassment at "the virtual obliteration from the contemporary philosophical mind of a whole philosophical tradition"—namely that of English Idealism. One would perhaps fairly express his sentiments by saying that even if one disagrees with a certain philosophical tradition, it is rash to ignore it completely when so many competent thinkers have in the past regarded its discussions and answers as worthy of serious consideration. I agree; and I think that in his first course Campbell proves his point. I would make precisely the same point about the scholastic tradition, and I think that Campbell's second course proves mine. He attacks "orthodox or official theism" for a number of errors—for attributing will and knowledge to God in a sense that inconsistently involves God in imperfection, for holding, as a result of the doctrine of the Fall, that man is utterly depraved and devoid of that natural tendency to good which moral consciousness assures us that we possess, for teaching the doctrine of creation in a sense that makes the act of creation a temporal act, and so on. Now whether or not scholasticism may claim to represent "orthodox or official theism" I shall not debate; it must at least be admitted as a matter of pure history that the Aristotelian-Platonic synthesis of St. Thomas has been virtually the only large-scale metaphysical system to gain a consistent following of Christian thinkers, not by any means all Roman Catholic, for a number of centuries; and since it seems to me that even a cursory examination of St. Thomas's writings would show that he did not commit the errors against which Campbell inveighs, it would seem at least as tragic to ignore Thomism as to ignore Bradleian idealism, at least if one's subject happens to be natural theology.

The one mention of scholastic theories is a summary of the doctrine of analogy as presented by Dr. E. L. Mascall in his excellent *Existence and Analogy*, which Campbell adds in an appendix for the sake of comparison with his own version of symbolism. In some ways the two theories are so close in their end results that it seems additionally unfortunate that Campbell did not consider the scholastic doctrine critically in his text; I have already indicated by implication the deep metaphysical differences between the two approaches. Where the scholastic seems to me to gain over Campbell's on the specific point of knowledge of God is that for Campbell we can only tell, at most, *that* and not *how* the theological symbol is related to its divine symbolizandum, for on his theory, derived from Otto, the analogy is simply a felt analogy of emotion. We have not the metaphysical basis of the creator-creature relationship, nor a recognition of the analogical nature of such terms as 'being' and 'good' even in their empirical employment, on which to base any rational explication of the analogy.

If Campbell ignores an important philosophical tradition, he also ignores some highly relevant aspects of traditional Christian belief. He is quite justified in pointing out that it is not the duty of a Gifford lecturer to defend a specific faith, but his book would have been much more valuable if he had at least taken accurate cognisance of the central teachings of that form of theism in which his audience is most likely to be interested. Take, for example, the somewhat Cartesian chapter proving that the self is not essentially embodied. Quite apart from the fact that he appears to aim invalid arguments at a theory whose conclusion does not follow from its premises (one could not prove that the self is *essentially* embodied by any mere appeal to introspection), he ignores the really strong arguments against his position, e.g. the rooting of thought in the bodily senses, and the traditional answers to them; but consideration of those arguments might have led him closer to the traditional Christian teaching that in man the soul demands a body for its completion and is destined to be re-united with it, in some form, at the resurrection.

But perhaps part of the reason for this eclectic attitude to religion—as well as an example of it—is Campbell's view of the relation of reason and revelation. Ignoring the possibility of revelation through the Person of Christ, the Bible or the Church, Campbell concentrates on revelation through personal religious experience; and in this sphere it is very plausible to hold that reason is the ultimate arbiter both of the channel and of the content of alleged revelations. But there is another possibility which Campbell does not squarely face: that reason is the ultimate arbiter of the medium, but that having given its allegiance to a particular medium, reason may then find herself bound to adopt a creaturely humility and lay herself open to a truth beyond her powers of adjudication. Not to believe that there is in fact any divinely accredited medium of revelation is a tenable opinion; but not to consider the possibility and the

implications of there being such a medium is a philosophical omission, surprising in one who writes against a Christian background and especially so in a convinced supra-rationalist. Campbell seems not to see that one cannot validate the authority of a revelatory medium purely by induction—i.e. by adjudicating severally upon its pronouncements—without rendering the whole idea of revelation nugatory.

The evident fairness and acumen which Campbell displays throughout—and which my adverse comments may seem to have neglected—increase my disappointment with his book; it could have been so much better if its philosophical horizons were not so circumscribed.

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IX.—NEW BOOKS

Pap, Arthur: *Analytische Erkenntnislehre*. Vienna, 1955. Pp. viii + 242.
£2 1s.

THIS book is fundamentally an act of piety, a squaring of accounts, the carrying of coals to a Newcastle whose mines are apparently exhausted. It emerged in a rather complicated and circuitous way from lectures given by Professor Pap as visiting professor at Vienna in 1953-54, most of it being reconstructed from very complete notes taken by Dr. Feyerabend. There are no obvious marks of the lecture-room about the resulting text, no traces of chattiness, humour or conciliatory irrelevance. What it adds up to is a fairly thorough examination of six main fields of interest to positivist philosophers: verification, phenomenalism, truth and probability, causality, explanation and necessity. Pap says that a book in German on analytic philosophy is needed since it is now extinct and almost forgotten in its birthplace. He does not pretend to completeness but aims only to give some indication of the 'subtlety and thoroughness' of the analytic procedure. We are not to infer from the book's dedication to the Vienna Circle that its author accepts all their doctrines; what he does support is their conception of philosophical method.

These preparatory observations are somewhat misleading. They suggest that what follows is essentially an exposition and defence of the leading doctrines of logical positivism combined with a certain amount of critical reservation, qualification and revision. In fact the characteristic theses of the school are generally taken very much for granted and little in the way of positive support for them is produced. Instead there is a running fire of rather disjointed critical comment. If this were somebody's principal introduction to analytic philosophy he might well think it a tissue of incoherencies, and he would certainly not acquire any clear idea of how its views hang together. To start with, the topics are not treated in any natural order. Perhaps the order chosen could be justified but no reason for it is given. Practically nothing is said about the historical background and development of the doctrines criticised. Pap seldom summarises the results of his criticisms. After producing a series of refutations of some doctrine he just stops, without asking whether anything is left of it, whether anything at all similar could be reconstructed from the remains or whether there was any good reason for advancing the doctrine in the first place. It all seems to come very much from the front of his mind, to emerge from the immediate preoccupations of someone working inside a positivistic tradition. What German readers, brought up on a diet of *Sprachräumerei*, will make of it is, then, open to question but it is of some interest as a report on the present state of opinion in the Carnapian or formalist sect of the analytic church. As such it is lucid and authoritative but one does feel that only a small part of the work has been done. The point of refutation is to clear the ground for new theories, and new theories are conspicuously absent from Pap's book, except in the form of vague intimations of a more or less transcendentalist variety (e.g. may not a causal law assert an intensional connexion of predicates, does not an adequate definition record an intuition of conceptual identity?).

We start off with the verification criterion of significance. What sort of possibility of verification is involved? If, as is usually maintained,

this is logical possibility, difficulties ensue. For unless the assertion being examined is straightforwardly selfcontradictory, a relatively uninteresting case, to decide that its verification is logically impossible presupposes the discovery that it expresses no proposition, is cognitively meaningless. Are analytic statements senseless? If verifiability is limited to synthetic statements a further circularity is revealed. A sentence is only synthetic by virtue of expressing a synthetic proposition, but only significant sentences express propositions, therefore syntheticness presupposes significance. These demonstrations of circularity have a scholastic flavour and seem to depend on construing identity as presupposition. We don't first have to decide that a sentence is synthetic and *then* go on to see if it is verifiable. To find that a sentence is associated with a method of empirical verification is to find that it expresses a synthetic proposition. If there is no such associated method the question remains open as to whether it is analytic or 'cognitively meaningless' (*i.e.* not a statement, not the sort of remark that can be judged to be true or false). A psychologistic criterion in terms of conceivability is rejected. The usual watering-down from verifiability to confirmability is argued for to accommodate the significance of statements about the unobserved. But behaviouristic and futuristic manoeuvres with statements about other minds and the past are declared to be unnecessary. Propositions are impersonal and timeless, it is sentences that are classified by reference to other minds or the past.

The remainder of the chapter is taken up with consideration of Hempel's doctrine that significance should be assessed by reference to translatability into an extensional empirical language and with various problems arising therefrom, in particular with the refractoriness of modal statements and belief statements.

The ensuing chapter on perception is dedicated to the dissolution of phenomenalism all of whose characteristic theses are given a thorough pasting. The first difficulty arises about the nature of the hypotheticals that make up the phenomenalist translation. Pap suggests that they should be interpreted as probability-implications. What, he next inquires, is the relation between the translated statements about objects and the sensory hypotheticals into which they are transformed? It is neither straight logical equivalence nor mere empirical association but a probability-relationship where the probability involved is not of the frequency type. From this he concludes that the realism-versus-phenomenalism issue loses its point. The two main assumptions from which the whole phenomenalist project derives are then inspected and condemned, namely that all statements about objects are uncertain and that there are some incorrigible basic statements.

Against Tarski's semantic definition of truth Pap's beloved sentence-proposition distinction is once more brought into play. To say that the sentence 'snow is white' is true if and only if snow is white is to make a contingent assertion about the actual use of the form of words 'snow is white'. A fairly conventional discussion of probability follows. Probability, unlike truth, is relative. The formal calculus that treats of the relations of one probability-statement to another is firmly distinguished from the interpretation of single probability-statements. The general identification of probability with frequency is rejected on two grounds: frequency-statements may themselves be more or less probable and the probability of theories cannot be based on the truth-frequency of their verifiable consequences. Carnap's resuscitation of the logical relation theory, with its un compelling assumption of the equiprobability of

structure-descriptions, is respectfully handled though the question is raised of its applicability to natural languages. Pap divides approaches to the problem of induction into those which rely on some principle of the uniformity of nature, those which proclaim it to be a pseudo-problem and those (*viz.* Carnap's and Reichenbach's) which aim to give an 'analytic' justification of inductive procedures. The Carnapian view that ordinary probability-inferences are the material conditions of adequacy for the definition of 'probable' looks like a weak version of the Wittgensteinian view put forward by those who are assigned to the pseudo-problem category.

Philo of Megara's uneasy shade hovers menacingly over the discussion of causality and dispositions. The vacuous truth of uninstantiated general material conditionals is crucial to certain formal objections lodged against the regularity theory of causation. Determinism is assigned a heuristic function and said to be a maxim about the form of theories. Pap is so convinced of the insufficiency of extensional solutions in the trampled field of dispositions, natural laws and counterfactuals that he suggests, but does not develop, an interpretation in terms of the intensional connection of predicates. Carnap's technique of reduction-sentences is rejected on the ground that sugar would still be soluble in a world without liquids. Pap follows Hempel and Oppenheim fairly closely in his account of explanation. But he raises some doubts about the concept of observability in terms of which the distinction between theoretical and instancial or subsumptive hypotheses is commonly drawn. No doubt, he says, electrons are unobservable, but then so are tables by comparison with sense-data. He does not connect this discussion of observability with anything he has earlier had to say about the problem of perception. The apparently teleological nature of human conduct is dealt with by asserting motives to be a kind of cause. The question whether there are any emergent qualities in nature is transformed into the question whether any predicates must be ostensively defined. His conclusion that no *particular* predicate must be so defined is not, however, equivalent to the proposition that no predicates whatever need to be defined ostensively. If we can predict the smell of some new chemical compound this can only be in the light of some general law relating chemical composition and smells that rests on observed instances of their contingent association. Physicalism in the philosophy of mind is, Pap maintains, an empirical thesis and one that comes to grief on the facts of self-control and acting. Ryle's dispositional account of mental states is refuted by the introspective certainty we have of some of them.

The last chapter on logical necessity brings together the ideas that Pap has expressed on this subject in a number of articles. He claims that 'all *a priori* statements are analytic' is an inductive generalisation, and one that has exceptions. Following the Frege-Quine definition of 'analytic' as 'that which is definitionally reducible to a law of logic' he concentrates, as Quine has, though with different intent, on the reference in it to definition. What, he asks, is an adequate definition? He decides that it is an intuited equivalence of concepts and not an empirical statement about words. For whatever we may use the expressions 'bachelor' and 'unmarried man' to mean the *concepts* they are now used to express are eternally the same. Cases are enumerated that while necessarily true are not reducible in accordance with the Frege-Quine definition ('nothing can be red and green all over', 'red is a colour'). Pap argues that the applicability in certain domains of non-Euclidean geometry does not refute Kant's view of the nature of mathematical truth since Euclidean

geometry is intuitively true of perceived, phenomenal, space! How is he so sure that two phenomenal straight lines cannot enclose a phenomenal space? And how does one discover that the angular sum of a phenomenal triangle is 180 degrees? Pap goes on to say that only the logistic thesis adequately refutes Kant's account of the nature of arithmetic and the logistic thesis is only acceptable if the adequacy of the logistic definitions of mathematical concepts as constructions out of narrowly logical concepts is intuited. Arithmetic is not proved to be analytic by our refusal to admit counter-examples to its laws. For we do not admit counter-examples to the law of the conservation of energy. Arithmetical theorems and energy laws are in the same boat: both functionally *a priori*. Against the 'linguistic theory', which Pap interprets as asserting statements to be analytic that follow from rules of language, he argues that they can only follow with the aid of logic and denies that the laws of logic are conventional. For how can a necessary connexion follow from contingent, temporary, facts about usage? Necessity is not a contingent property of sentences but a necessary property of propositions. He attempts a *reductio ad absurdum* of Quine's holism. The book concludes with a brief discussion of the paradox of analysis. The two main instruments with which Pap tries to overturn the analytic theory of necessity are his doctrine of adequate definition as the intuited equivalence of concepts and his conception of necessity as a timeless property of propositions. He is well aware that his point of view will be attacked as a hypostatisation of meanings but is curiously indifferent to the detailed articulation of this line of criticism. If the expression 'A' means the same as the expression 'B', if 'A equals B' is an adequate definition, this is not *because* the two expressions express two equivalent concepts. To say that they mean the same is just the same thing as to say that they express one and the same concept. Conceptual identity is not a *condition* of definitional adequacy, it is the essence of what a definition, claiming to be adequate, asserts.

This is already a long review but it has mentioned only a small selection of the topics that Pap discusses. Admirable as this wealth of detail is, much of it could well have been spared for the sake of greater thoroughness and greater organisation. Anyone interested in the subjects examined will have his ideas turned over in a hygienic and useful fashion by reading this book but the really serious work he will have to do for himself and while he will carry away a lively impression of Pap's dissatisfaction with some of the main articles of the positivist faith he will have received little assistance in the task of working out an alternative to them.

ANTHONY QUINTON

Foundations of Inductive Logic. By R. F. HARROD. Macmillan & Co. Ltd., 1956. Pp. xviii + 290. 24s.

MR. HARROD claims to show "what the essential principles of induction are" and "that these principles are co-equal in validity and authority and certainty with the principles of deduction" (p. vi) or, as he puts it later "that there are forms of fully valid inductive argument that require no support from any presupposition as regards uniformity nor indeed as regards any characteristic of nature whatever" (p. 13). In short, he claims to have found a justification for a form of induction which is the basis of all our predictions about the future and, indirectly, of all other

kinds of induction. If, in this review, I concentrate on showing why I think his justification fails this is not meant to suggest that he does not have interesting things to say about such related matters as probability, sampling, laws of nature and memory.

The basic form of induction is what Harrod calls "simple induction", which must not be confused with induction by simple enumeration. His justification depends on his "Principle of Experience" which is that "if one is journeying over an expanse, but in total ignorance of whereabouts on it one is, one is unlikely to be on the extreme edge of it" (p. 63) or that "the mere fact that things have been found in experience to be thus and thus, gives in and by itself, a valid reason for holding that they will continue to be thus and thus for the time being" (p. 50). "Unlikely" is here to be interpreted as meaning "improbable" in the sense of a frequency theory such that "we are unlikely to be on the extreme edge" means that "if we continuously believe that we are on the extreme edge, it is certain that we shall be wrong much more often than we are right" (p. 246).

Harrod uses an extended analogy of a nescient traveller journeying over a uniform expanse, *e.g.* a strip of uniform colour, and continuously predicting that the expanse will go on for a given fraction of the distance for which it has gone on already. This traveller, knowing only that he has travelled some distance along a continuity, is able to arrive at a probability figure for the correctness of his predictions about the future. If, for example, he predicts continuously during the whole of his journey that the continuity will hold for one-tenth of the distance for which it has held so far, then it is quite certain (L-true) that over the whole journey he will be right ten times for every once he is wrong. He will be right during the first ten-elevenths of his journey, wrong during the last eleventh. Generally, if from the beginning he predicts a continuance of $1/x$ of the distance already travelled he will be right x times for every once he is wrong. That is, the overall probability of being correct is $x/x+1$, and the more modest the extrapolation the higher the probability of its being correct. This fraction is the ratio of all correct predictions to all predictions, as surveyed at the beginning of the journey, whatever the length of the journey. He can be sure of all this from the very beginning of the journey.

However, the probability of predicting correctly at any point after the journey has begun must be arrived at by subtracting the predictions already made, since what is required is the ratio of all correct *future* predictions to all future predictions. The traveller, being nescient, cannot know this ratio, because he knows nothing about the distance still to be travelled, but Harrod has a device for overcoming this difficulty. If we again consider the whole journey, the ratio of all correct predictions to all predictions in the array, after subtracting predictions already made, considered from each point of view, Harrod shows to be $x^2/(x+1)^2$, whatever the length of the journey. This ratio can be arrived at by the traveller at the beginning without any assumptions about nature or about the length of the journey. Now the traveller can and must, at each point in his journey regard this ratio as the probability of his prediction at that point being correct. Thus he is able to assess the probability of any of his predictions being correct and, if his predictions are modest, this probability may be high. If, for instance, he predicts a continuance of $1/100$, the probability of his being correct is $10,000/10,201$ at every point at which he makes this prediction. This is said to justify his prediction and, indeed, any induction of this sort.

Apart from the debatable question whether the traveller's problem is a

good analogy for the inductive problem, this last step seems to me to be highly suspect. Harrod anticipates (p. 60) the objection that if we survey the whole of the journey and the whole array of predictions we can see that this estimate of probability is misleading if it is accepted at any given point after the journey has begun and becomes more misleading as the journey proceeds if the continuity is finite. The fraction $x^2/(x+1)^2$ is as near as the traveller can get to the ratio of all correct future predictions to all future predictions at any point. But we can see that whereas $x^2/(x+1)^2$ remains constant the ratio of all correct future predictions to all future predictions diminishes, and more and more rapidly as the journey proceeds. The error would rapidly become large. However, the rate of this decrease can be known only if the distance still to be travelled is known and any attempt by the traveller to base his estimate of probability upon an estimate of the distance still to be covered would be, for him, mere guesswork. Harrod concludes that his traveller can, in his state of ignorance, do nothing but hold to the fraction $x^2/(x+1)^2$ throughout the journey. This is the only estimate of probability which he is able to make, for he has no evidence upon which to base any other. But if this estimate is considerably out, as even the traveller can see that, for much of the journey, it must be, then surely he ought not to make an estimate at all and the whole justification falls to the ground. Indeed, put in this way, the problem of induction is just the problem of discovering, at any given point on the journey, how far this estimate is likely to be in error. If we were to show that the traveller could know the length of the journey we should, of course, prove too much, but a justification of induction demands that he should at least be able to make some estimate of the probable length of the journey. This has not been shown to be possible and it is difficult to see how it could be.

Harrod's continuity is intended to be analogous to the order of events, "the reign of law" in nature. We find ourselves some way along this continuity. Our predictions involve the fundamental one that this reign of law will continue. Harrod's fraction for the probability of its correctness would have been correct at the beginning of our journey but how far out is it now and how will our backward glance help us to answer this question? To say that we *can* put forward this estimate of probability now is surely fruitless since we can have no information whatever about the chances of its being correct now.

The Principle of Experience in its more picturesque form is undoubtedly persuasive. At first sight it is difficult to deny that if we may be anywhere on an unknown expanse we are unlikely to be on its extreme edge. This is, however, only true of certain kinds of expanse and therefore appears to make an assumption about nature. But even if we make this assumption, is it reasonable to conclude, on the basis of the Principle, that if we don't know where we are it is safe to bet that our next step will not take us off the expanse? I suggest that such a conclusion is not reasonable because it confuses two probabilities. It confuses the probability, given that we may be anywhere on the expanse, of our being in such a position that the next step will take us off it with the probability, given that we are at a particular point on the expanse, of the next step taking us off it.

Let me illustrate this with the help of a chessboard, where each square represents a "possible position", and a piece which can move one square (one step) at a time, forward or back, or left or right, but not diagonally. Now if this piece may be anywhere on the board, the probability of its being in such a position that one step will take it off is 28/64 and the

probability of its being in such a position that one step cannot take it off is 36/64. This latter probability increases, and the former decreases, with an increase in the number of squares on the board. But if the piece is on the board, the probability of one step taking it off will depend on its actual position. If it is on a corner square this probability is $1/2$; if it is on an outside square but not at a corner the probability is $1/4$ and if it is not on an outside square the probability is 0. These probabilities are independent of the number of squares into which the expanse is divided. The probability of the piece being in such a position that the probability of its moving off the expanse is low is therefore not the same as the probability of its moving off from a given position.

With this in mind let us return to Harrod's traveller on a continuity. Considering all possible positions, the probability of his being near the end of the continuity is low, if he may be anywhere on it. But a traveller on the continuity is in fact at a given point and the probability of its failing to continue beyond this point depends on the position of this point and may be very high. How can he possibly justify his prediction that it will continue by pointing out either that if he had made this prediction earlier he can now see that he would have been justified or that if he had said at the beginning that most of his future predictions during this journey would be fulfilled he would have been justified? This applies to every prediction in the series if he never knows where he is on the continuity and cannot even estimate his probable position.

This is analogous to our situation in relation to the reign of law in the natural world. Whenever we predict its continuance we may be near the end of the reign and the probability of our prediction being fulfilled depends upon how near we are. We are not assisted in estimating this probability by the estimate, which we *can* make, of the probability of the majority of our predictions in the whole series being correct.

Harrod is, I think, guilty of slipping about between the viewpoint of the nescient man predicting future continuance and that of the man surveying the total array of such predictions, knowing the length of the journey. The problem of induction which he claims to solve is the problem of justifying predictions to the effect that the reign of law will continue. To do this he must show that at any point at which we predict it is possible to say whether we are more likely to be right than wrong. But the fact that in the whole array of predictions there must be more correct than incorrect ones gives us no guidance on this matter. It therefore seems to me that Harrod has failed to substantiate his claim.

I have noticed one serious misprint. On p. 57, three lines from the bottom "false" should be deleted.

PETER ALEXANDER

Ockham. *Philosophical Writings: a Selection edited and translated by PHILOTHEUS BOEHNER*. Edinburgh, Nelson, 1957 ("The Nelson Philosophical Texts"). Pp. lix + 154 + 147. Price 21s.

THIS small volume, accurately and clearly printed, makes for the first time accessible to a wide public some of the more significant passages in Ockham's works: some of them had never been printed before, others only in A.D. 1495-1496. The selection consists of thirty-one extracts from seven works: fifteen—covering about fifty pages—from the longest of Ockham's three summaries of logic (*Summa Totius Logice*), fifteen—about seventy pages—from his discussion on Aristotle's *Physics* (*Expositio et Questiones*),

on Peter Lombard's theological compilation (*Ordinatio* and *Reportatio*), and on miscellaneous topics (*Quodlibeta*), and one of two pages from the *Expositio* on Aristotle's *De interpretatione*. An adequate English translation to which Mr A. Wolter and Mr P. T. Geach have also contributed faces the Latin texts. The introduction by the late Ph. Boehner includes a concise life of Ockham and an account of some among his views, mainly of those expounded in or implied by the selected passages. The account is on the whole clear; it is misleading when it seems to suggest that Ockham himself was responsible for distinctions and doctrines current for several generations before him, such as those concerning the first and second imposition and intention of terms, categorematic and syncategorematic words, *suppositio*, and the threefold division of logic. An exhaustive list of Ockham's philosophical and theological works is accompanied by some useful information on their composition, chronology, and editions; the list of recent literature on the subject is intelligently selective. The indexes could be improved by cutting down references to the introduction and increasing those to the texts. A better division into paragraphs and a better punctuation would make some of the extracts more readable than they are now.

The choice and arrangement of passages were dictated by present-day philosophical and theological interests and worries. But Boehner was not always successful in his attempt to fit into his scheme of sections—epistemology, logical problems, doctrine of *suppositio*, truth, being, natural theology, physics, and ethics—bits of works which were originally arranged in a different, maybe less haphazard, order. Thus, Ockham's sympathetic exposition of a doctrine according to which knowledge of quantities implies knowledge of the things of which they are quantities—as against what happens in the case of qualities—appears in the Physics section together with a discussion of the problem whether the soul can be proved to be at the same time the ghost in, and the functions of, the body. A defence of arguments from authority is included in a section devoted to rules of propositional calculus. The only passage coming under the heading of Ethics is given the title "The basis of morality": in fact, it is essentially a discussion of a paradox: "I must love God by obeying him; but God may command that I should not love him; thus, if I obey I disobey, and if I disobey I obey." Ockham's political and ecclesiastical writings, from which his views on the interpretation of laws and authoritative texts as well as those on social and moral problems could be extracted, have not been drawn upon for this selection, perhaps because they are just now being newly edited.¹

The following points of doctrine stand out in the chosen texts: (a) *scientia* is a mode of being of the mind, "*qualitas existens subiective in anima*"; (b) 'unity' in 'unity of a science' only implies that more prominence is given in that science to one concept as against all other concepts with which it is concerned; *physica*, e.g. gives more prominence to the concept of changing-thing; (c) science is directly concerned with concepts, and only indirectly with things; (d) the intellect can grasp things *qua* existing, in some cases even without the help of sense-experience; without sense-experience, it can also grasp things *qua* non-existing or free from existential qualifications; (e) 'the universal X' = 'X being

¹ GUILLELMI DE OCKHAM, *Opera Politica* accuraverunt J. G. Sikes†, R. F. Bennett, H. S. Offler. Vol. i containing the *Octo Questiones* and the *Opus Nonaginta Dierum*, i-vi, and vol. iii containing the *Contra Ioannem* and *Contra Benedictum* have already been published, Manchester University Press, 1940 and 1956.

conceived'; alternatively 'the universal *X*' = 'conceiving *X*'; (f) a proposition is true when subject and predicate refer to the same thing; (g) 'the essence of *a*' = 'the existing thing *a*'; (h) some conclusions can be equally reached from verifiable propositions and from statements accepted by faith; (i) the chain of causes bringing about something actual may be infinite, but the chain of causes preserving the same thing in existence must be finite; (j) 'God = the being greater than all other beings' does not imply 'God exists', while 'God = greatest among beings' implies 'an indefinite number of Gods exist'; (k) 'God is the cause of everything' and 'God knows future contingents' belong to the languages of faith and probability.

Most of what is included in the sections "Logical problems", "The theory of *suppositio*", and "Inferential operations" is more interesting as an exemplification of some philosophical learning and teaching going on in the late thirteenth and fourteenth century than for Ockham's contributions in these matters. None of the passages in which Ockham openly expresses his view that God can make and preserve catless Cheshire-cat grins, "*accidentia sine substantia*", is included in Boehner's selection.

Ockham's frequent care and illuminating accuracy in distinguishing the different meanings of words of the common language of the schools, such as '*scientia*, *subiectum*, *materia*, *forma*, *abstractivum*, *terminus*, *ens*', are not matched by a sufficient perspicuity in his argumentations: when he syllogizes—and there is much of this in the passages chosen by Boehner—he is often involved and clumsy. He adopts all the traditional distinctions concerning the differences in meaning and reference of words, and the propriety of exactness of their use: '*large—stricte, proprie* (or *de virtute sermonis*)—*improprie*', and the various kinds of univocity and equivocity, are instruments skilfully handled by Ockham for the purpose of clarification. In the twelfth and thirteenth century doctrine of the threefold reference of terms—to the words themselves (*suppositio materialis*: 'man'), to the concepts (*suppositio simplex*: Man), and to the things (*suppositio personalis*: this man, that man)—he found a particularly useful instrument for his attempt to simplify the structure of the world of knowledge, to exorcize from the world of things the ghosts of universals and from both worlds the ghosts of essences, and to expose the fallacies implicit in some paradoxes. The audacity of some of his doctrines or expressions is often balanced by cautious timidity: "*est alia opinio de quantitate que michi videtur esse de mente Aristotelis, sive sit heretica sive catholica, quam volo nunc recitare quamvis nolim eam asserere*" (p. 137). He tried to dismiss the compromise implied by *analogia entis* which should allow us to talk of God without properly knowing what we are talking about, by reducing analogy to one kind of univocity—"one concept of being can be meaningfully applied to all things including God"—but, lest he should have to go too far beyond the limits of an acceptable orthodoxy, he immediately fell between the two stools of univocity and equivocity (p. 108).

L. MINIO-PALUELLO

The Artist as Creator. By MILTON C. NAHM. Johns Hopkins Press, 1956. Pp. xii + 352. \$5.50.

THIS is a sequel to the author's *Aesthetic Experience* (1946); its subtitle is *An Essay on Human Freedom*, and Professor Nahm hopes to shed some light on human freedom in general by eliciting the main features of

the artist's freedom. In what sense is the artist free in creating his works of art? Nahm first considers the 'Great Analogy'; the artist is like the Creator creating the world. If by this be meant that the artist has absolute freedom in the sense in which the Creator is conceived to have had absolute freedom, the analogy soon fails us. (By Coleridge's time the creativity of the artist had become a 'dim analogue to creation'.) If the meaning is that something like the divine creative power enters the artist, possibly a daemonic or demiurgic power, then the artist himself, being possessed, is the least free of mortals. Nahm rejects the view that artistic creation can only be explained in this way; 'the creativity of the artist', he remarks, 'is explicable on natural grounds'. One could agree with this if the word 'natural' is used widely enough. For instance it would have to cover the precocity of a Mozart and such strange occurrences as Rilke's when the first line of a poem came to him out of the sea. On the other hand it is wise not to be carried away too readily by talk about inspiration. Some people speak of artistic creation as the fruit of a possession almost akin to madness. But this becomes absurd if one attempts to apply this theory, say, to so supreme, but also so sane, an artist as Shakespeare.

In Part II of this work, the more important part, Nahm considers 'on natural grounds' the creativity of the artist. And here he thinks the fundamental problem can be set forward in this question: How can the object of fine art be intelligible and significant and yet unique and wholly individual? Intelligibility and uniqueness are two essential characteristics of any object of fine art, and to understand how an object having these two characteristics can be created is to understand artistic creation and artistic freedom. Finding an answer to this problem will help, too, in the solution of many problems which face the contemporary philosopher of art, such as whether the activity of the artist is unfettered imagination or imaginative intuition which is expression in the Crocean sense; whether technique is essential; whether we can classify works of art and by what principles; whether there is a distinction between art and craft; whether any satisfactory account can be given of the relations between the beautiful, the sublime and the ugly, and so on. Nahm deals with all these problems in discussing his central topic.

The account which follows of Nahm's answer to this central question will necessarily be brief, though not, I hope, misleading. (I must confess that sometimes I am not sure whether I have wholly understood his position, and that partly because he frequently sets down his conclusions without revealing the thinking and reasoning by which he establishes them.) A work of art, Professor Nahm holds, cannot be adequately described by saying that it is something made by man, an artefact; nor is it enough to say that it is something which is significant to its creator and to others; nor, finally, that it is an expression of the artist, whether inward in imaginative intuition or outward in the object of art. To concentrate on one of these, to the exclusion of the others, is to fall into the error of over-simplification and into the consequent error of supposing for instance, that the artistic process is irrational, or that art needs no technique, or that it is never genuinely individual. On the other hand, a synthesis of the three points does give an adequate description. The work of art is 'concrete significant form'. It is made of a material according to a certain technique; it has significance; and it is an expression. Discussions of technique are not irrelevant to aesthetics, neither are the classifications of works of art which presuppose the presence in them of universal features which in turn make them intelligible.

Certain artefacts have in addition to the characteristics mentioned the further character of uniqueness and originality and these are the works of *fine art*. In their creation the individuality of their creator is given freedom of expression; what is valuable in his eyes, what ought to be, is made manifest. The contemplation of such works of art by others capable of artistic appreciation stirs up in them feelings of their own originality and Nahm speaks pregnantly in this connection of 'fine or freeing art'. I am not sure, however, that this provides an adequate basis for a distinction between a work of art and a work of fine art. A man may make something that reveals his individuality and is individual in itself, it may reflect too his values, and yet may not be a work of fine art. But Nahm's argument here is not entirely clear to me.

However, he makes one all-important point abundantly clear. The artist's creation, even when we speak of fine art, is not absolutely free creation in the sense, first, that it creates *ex nihilo*, and, secondly, that it lacks rules and needs no technique. In so far as the Croce-Collingwood school adopts such a view of artistic freedom Nahm emphatically rejects it. The artist's freedom in creation is a conditional, limited freedom. It is limited by the material upon which the artist works. It is true that genius, as Kant said, sets down its own rules, but they are not arbitrary rules. They are conditioned by the material. The artist is original in the way the material allows him to be. Moreover, he is limited in other ways, for instance, by his social milieu, by its traditions and culture. If he is free, and Nahm certainly thinks that he is, it is a freedom within limits. There is imaginative intuition; it is quite true that the artist need not know beforehand what he is about to create. The blueprint is an unsatisfactory analogy; but so also is the 'great analogy' of the absolutely free Creator. It would tally well with the spirit of Nahm's book to speak of artistic creation as sometimes analogous to the growth of plants. We speak significantly of the 'flowering' of artistic genius. There is a period of gestation and of silent growth, what is coming into being is influenced and moulded by what surrounds it and yet it is its own unique organic self. Perhaps the biologist can provide the philosopher of art with analogies as useful and as helpful as the theologian and the planner.

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XI.—NOTES

JOINT SESSION WITH THE ARISTOTELIAN SOCIETY 1959 AND GENERAL MEETING

The Joint Session will be held at St. Andrews on the 10th–13th July 1959. Information and application forms will be sent automatically only to members resident in Europe [List (a) in this issue.] If any member not resident in Europe would like to have those papers or to buy the Supplementary Volume of the Proceedings of the Aristotelian Society containing the papers to be read at this Joint Session he should write to the Treasurer, Mr. J. D. MABBOTT, St. John's College, Oxford, before the end of April.

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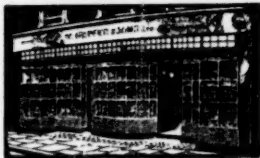
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